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COURSE OF LECTURES
ON
DISEASES OF THE SKIN;
THEIR
HISTORY, PATHOLOGY, AND TREATMENT.

Delivered in the Middlesex Hospital, during the Summer Session, 1842, *by*

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LECTURE XVI.

EFFICACY OF VACCINATION AS A PROTECTION AGAINST SMALL-POX.

WHEN vaccination is perfect there can be no doubt as to its power of protecting the vaccinated individual against the dangers of small-pox, either by preventing its invasion or controlling its effects. This may be regarded as a general law, but, like all vital laws, liable to occasional exception, to be overpowered, if I may so speak, by some other law stronger than it. Indeed, the exception is so well established as to be itself a general principle, and to form a necessary reservation when we allude to the operation of general vital laws. Small-pox after small-pox is too common a case at the present day to excite our wonder, and yet it is a general law that small-pox attacks an individual but once in his lifetime. Small-pox after vaccination does also occur, but not more frequently than small-pox after small-pox; the general law is consequently as active in the former as in the latter case. If we inquire, What are the causes of the general exception? we are forced to admit our inability to determine that question, and we can but reply, that the secret springs of vital influence, doubtless for a wise purpose, are hidden from our search. It is my intention, therefore, to impress on your minds only the general law—which in my opinion is not the less important, because not universal—that vaccination, when perfect, is as complete a protection against small-pox as any other power known to man.

You will perceive that I lay great stress on the perfection of the operation, for it would be unreasonable to expect that perfect results should flow from imperfect sources; and viewing the manner in which the operation is received among the poorer classes, you cannot but agree with me that in many cases it cannot be otherwise than imperfect. That vaccination be perfect it is necessary,

1. That the lymph employed in the operation be pure.

2. That it shall have been procured from a vesicle that has passed regularly through the stages described in my last lecture.

3. That it shall have been obtained from the vesicle between the fifth and the eighth day of its course.

4. That the pock produced by the lymph in the vaccinated person shall have passed regularly through the stages known as the natural course of the vaccine-pock.

5. That at least one of the vesicles produced by vaccination shall have been permitted to remain unbroken and uninjured, until the vaccine crust shall have been formed, and shall have fallen in the natural course.

6. That the cicatrix shall be well marked and permanent; perhaps, also, foveolated.

These conditions, you will perceive, imply that which is the fact, namely, that imperfection in vaccination is by no means unfrequent, and that imperfection may attend any one of its stages; but the most important variety is that in which the vesicle is modified, either in size or form, in its contents, or in the proportion of inflammation by which it is accompanied. Sometimes the vesicle is smaller, at other times larger than usual; its form may be irregular in place of the natural symmetry of the pock, and its contents too late transparent or too soon puriform. It has been said that the nearer the approach of the vesicle to the standard characters the greater will be the proportion of security to the patient; but this is a dangerous doctrine inasmuch as it may lead us to be satisfied with our work when only half done, and vaccination may be accused where our own carelessness is the real cause of want of success.

The presence of a large and well-defined cicatrix is a point of great importance in de-

termining the success of the operation in after life. Without a cicatrix it may be safely affirmed that vaccination has failed, and that consequently any statement relative to the performance of the operation is to be unheeded. If the cicatrix be foveolated it is still more satisfactory, but the absence of foveolation is no proof of the individual being unprotected.

VACCINATION TESTS.

There can be no question that the best test of the success of vaccination would be the exposure of the vaccinated person to the infection or contagion of small-pox, without any adverse results; and this is a test which has been frequently put to the proof during the epidemic prevalence of that disease. The vaccine influence has also been frequently tested by an opposite experiment; children have been vaccinated during the period of sickening for small-pox, at a time when they were surrounded with the infection of the latter disease, and the modifying power of the vaccine disorder has been shown in a striking and conspicuous manner. Cases illustrative of the remarkable effects of vaccination after small-pox has set in have been recorded by many practitioners, and vaccination is recommended and practised by Eichhorn as a means of treatment in small-pox.

To attain the information required with more certainty than could be deduced from exposure of the vaccinated individual to the infection of small-pox, it was proposed to inoculate after vaccination, and in those cases in which this operation was performed the inoculation failed, or was so modified as to have lost its natural characters. I was myself the subject of inoculation at about thirteen or fourteen years after vaccination. On the eighth day I was seized with heaviness and sleep, attended with febrile symptoms, and a trifling papular eruption, but in a few days was quite well. During this period I mingled with school-fellows and members of my own family, but no communication of the small-pox resulted.

Another test of the success of vaccination is revaccination, and this, as it determines at the same time the susceptibility of the individual to the variolous poison, is an important operation.

Another test was suggested by Dr. Bryce, of Edinburgh, and is one of considerable interest in a physiological point of view. It consists in revaccinating in the course of a few days after the first vaccination. The results are as follows:—If the previous vaccination have been ineffectual, the second will proceed gradually through the stages common to the ordinary vaccine pock. But if the first vaccination shall have been successful in engaging the entire system in the vaccine process, the pock produced by the second will pass rapidly over the stage of incubation and papulation, and take on the form corresponding with the constitutional period of the disorder. In other words, the second pock will attain an equal development with the first, both will proceed together to the acmé, and the processes of suppuration and desiccation will be simultaneous in both.

OTHER CONSIDERATIONS RELATIVE TO VACCINATION.

Vaccination is one of those simple operations that may be performed without any previous preparation of the patient; but there are, nevertheless, certain precautions to be used which demand the vigilance of observation which forms so conspicuous a feature in medical pursuits. Jenner remarked that there were certain cutaneous disorders which were calculated to affect the success of vaccination, and there is advantage in considering the influence of age in modifying the course of the vaccine disorder.

It would appear that there is no speciality in the cutaneous disorder which may be supposed to act as the cause of interference to vaccination, for different observers have mentioned disorders belonging to all the groups of inflammation of the dermis, namely, congestive, effusive, suppurative, depositive, squamous, and parasitic. The influence of cutaneous diseases is consequently counter-irritative or revulsive, and to this principle may be referred dentition and febrile diseases in general as impediments to the successful progress of the vaccine pock. It is interesting, however, to find it remarked by Jenner, that where the eruptive disorder presented a suppurative character, he observed that the vaccine-pock contained pus from its earliest appearance.

In determining the proper age for the performance of vaccination we have to bear in mind that infants are liable to be attacked with small-pox from the earliest moment of their birth. Nay, that even before birth they may be the subject of that disease, even when the parent is wholly unaffected. Dr. Mead has placed on record a case of this kind, and several have been reported by other practitioners. In these latter cases, however, it is clear that medicine can do nothing, save to point out to pregnant women the danger to their offspring of exposing themselves to the infection of small-pox.

After the birth of the child our conduct must be determined by existing circumstances; if small-pox be prevalent in the vicinity, we cannot vaccinate too early, and every other consideration must give way before the hope of protecting the child from the present invasion of so dreadful a disease. But when no such immediate danger is present or probable, we may inquire, What period is best suited for the performance of this operation? Many practitioners pursue

the practice of vaccinating between the third and the seventh month, a period when the first dangers of life are past, and antecedent to the sources of inconvenience and failure that originate in dentition. It may be said, also, that this period immediately precedes the exposure of the infant to the chances of infection, especially in large towns, and, therefore, as a general rule, is that best suited to the operation.

On the other hand, it is quite true that infants are less susceptible of contagion and infection than either children or adults, probably from the activity of their formative functions, and the quiescence of their nervous system. Hence they are less likely to take small-pox when exposed to its influence, and less affected by vaccination. This principle appears to be recognised by Mr. Ceeley, when, in seeking subjects best fitted to receive primary lymph, he remarks:—"Children are the best certainly for the purpose, and such should be selected as possess a thick, smooth, clear skin, and have a dark complexion, and are not too florid, but still plump, active, and healthy. Infants of this description are decidedly the best; their vesicles, we know, are almost always better defined, less liable to burst, or be broken, have less areola, and, in fact, if between four and nine months old, are capable of furnishing with this lymph all that can be desired with the least amount of risk." Apposite to the subject before us I find the following remarks in a very excellent practical volume on Vaccination by Dr. Labatt, of Dublin:—

"I have witnessed (says this author) many remarkable instances of new-born infants resisting the infection of scarlet fever, typhus, and other infectious complaints." "Children have been vaccinated a few hours after birth, and have gone through the disease in its usual mild form. I myself inoculated a child a few hours after birth, and another the day after it was born, and several within the nine days; of the propriety of which, however, I am now strongly disposed to doubt, unless the existence of small-pox contagion in the neighbourhood, or some such cogent reason, renders the operation immediately necessary. We should not, if possible, inoculate a child till it be at least a month old; and I believe it will be found that new-born infants do not receive vaccine infection so readily as those more advanced, at least this is consistent with my experience. The same was observed in the inoculation with small-pox." "Dr. Heim, in his treatise on small-pox and vaccination in Wurtemberg, gives the result of numerous cases tending to prove that cow-pock is not so well developed in early infancy; that in fact an immunity from cow-pock infection may, to a certain extent, then exist; and that, therefore, too early vaccination of children cannot be considered proper in all cases, and especially within the first year."

From an ample consideration of the phenomena of vaccination, I think, then, we may conclude that the operation may be performed at any period of infantile life, should circumstances render it advisable; and that when we have an opportunity of making choice, we may except the first two months, the period of dentition, and that of any febrile or cutaneous disease. But the subjects which we have heretofore discussed, raise a question of much higher importance to the success of vaccination than that of protection to infantile life alone. If it be granted that in infant life the system is less disposed to take on the vaccine ferment than in later childhood, it may be very properly deduced that the individual is less effectually protected by such vaccination than by later vaccination. This interesting question has been made the subject of several papers of great practical value from the pen of Mr. Grantham, of Crayford, who recommends that, all things favourable, vaccination should be postponed until after the ninth month of infantile life, and he makes this suggestion, supported by a "mass of facts." I think that we cannot do otherwise than agree to the principle suggested by Mr. Grantham, seeing that his purpose is that of extending the protecting influence of vaccination to as long a period of life as possible. But there are, as that gentleman admits, circumstances that demand a modification of his practice, such as a variolous epidemic, and to this, I think, we may add, residence in cities and large towns. For in the latter it may be said that small-pox is ever there, that infants are very early exposed to its dangers, and that protection should precede the possibility of infection. If I were called upon to offer an opinion on this question, I should say,—in country towns and villages protract the period of vaccination to as late a period as is consistent with safety, say to the sixth, ninth, or twelfth month, but in the metropolis the extreme period should be the end of the sixth month. Moreover, to anticipate the probably ill-consequences suggested by Mr. Grantham, let revaccination be performed at a subsequent period, either of later childhood or at puberty.

EXTENSION OF VACCINE PROTECTION,—REVACCINATION.

Whether vaccination, when properly and efficiently performed, be capable of prolonging its influence indefinitely, is a question of much importance, but one which cannot at present be answered. Were it, indeed, effectual, our sole anxiety would be to secure the perfection of vaccination; but, in a state of doubt, it becomes doubly necessary to be vigilant in our performance of the operation, and to take such steps as may best anticipate any probability of failure. We

have seen that the causes of interference in relation to vaccination are very numerous, that the subject vaccinated may be too young; that he may be in a state of health unfavourable to the success of the operation; or that the lymph may be incompetent, either as being obtained too late, or from an imperfect vesicle. With all these chances of failure before us, we shall not be surprised to find that an impression unfavourable to the powers of vaccination exists in the minds of practitioners.

For the purpose of meeting this difficulty, revaccination has been suggested, and on the continent extensively practised. Revaccination promises to us more than one advantage; it is a simple and harmless operation in itself, and if the vaccinated person be susceptible of its influence, it may be regarded as determining, firstly, the unprotected state of the individual; and, secondly, his future protection. An infection of whatever nature introduced into the animal system, induces such a modification in that system as to render it insusceptible of the future influence of that infection. But a question difficult of solution is, For what length of time is the system protected? The physiological changes that are known to be constantly taking place in the system, together with observation, would lead us to infer that the period of existence of the influence is variable; at one time, and more rarely, extending over the whole of life, but more frequently restricted to a limited period.

The admission of a possibility of the decline of the vaccine influence, leads us at once to the necessity for revaccination, and leaves no other question to determine than the periods at which the operation should be performed. I should advise that vaccination be repeated every seven or every ten years. If the system receive not the inoculated virus, it may be regarded as protected, and no inconvenience results to the subject of operation. While, on the other hand, if the operation be successful, the inconvenience will be temporary and trifling, but the advantages great.

You must bear in mind that the greatest safety against small-pox that man can enjoy is the possession of that modified constitution that succeeds to the fever of small-pox. Experience teaches that the amount of eruption is of little consequence, as much benefit flowing from the most benign as from the most confluent kind, from inoculated as from natural small-pox; and I have no hesitation in declaring that as much protection as small-pox can bestow is derivable from perfect vaccination. The following table, quoted from Dr. Heim, places small-pox and cow-pox in a very interesting light as regards the protection which they are capable of affording, namely,

Vaccinated after small-pox with success	32
„ modified	26
„ without effect	42
	100
Revaccinated with success	34
„ modified	25
„ without effect	41
	100

This table places small-pox and cow-pox on an equality, which I believe to be founded in truth; and any majority of cases of small-pox after vaccination over those of secondary small-pox, I should certainly attribute to imperfect vaccination. One of the most eminent physicians and writers of the present day, Dr. Robert Williams, entertains a different opinion on this question. He writes, "The expectation formed by Dr. Jenner that the cases of small-pox after vaccination would not be more numerous than the cases of secondary small-pox, has not altogether been verified, the evidence at present accumulated showing the attacks of the latter to be only in the ratio of half to one per cent., while the attacks of the former are not less than five per cent., or from five to ten times greater. It is enough of glory, however, to the discoverer of vaccination, and of honest pride to the profession who have adopted it, to be able to state, that by the discontinuance of the practice of inoculation the total number of persons attacked by natural small-pox in this country, taking the most unfavourable calculations, is reduced one-half, or probably from 260,000 annually, to about 130,000 annually; while the number of deaths have been reduced in a still greater ratio, or from 60,000 to about 11,000; also that the accidents incident to the disease, as blindness, deafness, lameness, and the endless catalogue of miseries which follow it, are likewise reduced almost to nothing. This result is that of England and Wales generally, and it is still capable of being greatly reduced, for among the better protected class of persons, as the army, only one soldier has been attacked by small-pox in every 2000 annually; so that taking the British army at 100,000 men, the mortality is only four from small-pox in the whole of that large force annually. The navy appears also to experience a similar immunity, for out of a mean strength of 7958 seamen, seven only died in seven years of small-pox in the Mediterranean and Peninsular commands; while, in the West Indian and North and South American commands, none whatever. On the continent, also, where the governments awakened to the great truth that the health and industry of the lower orders form the surest basis of national wealth and greatness, and where vaccination is consequently made of national importance and a matter of legislation, we find that the mortality from small-

pox, though greater than in our army, is infinitely less than in England and Wales generally. In Prussia, for example, according to the table given by Hoffmann, on an average of a million of deaths, only 8191 were caused by small-pox, or one in 122. In England and Wales, however, out of 141,607 deaths, 5811 were occasioned by small-pox, or one in twenty-five nearly; thus showing that the country that gave birth to vaccination suffers six times more by small-pox than that of its wiser and more considerate neighbour."

VARIOLATION AFTER VACCINATION.

Inoculation with small-pox after vaccination offers no advantages which are not to be derived from revaccination. We have seen that vaccination, when properly performed, is equal to an attack of small-pox, and is as little likely to be succeeded by small-pox as small-pox itself. We may regard vaccinia in another and its true light, namely, as identical with variola, and consequently the operation as the same with inoculation with small-pox; the only difference being the greater mildness of vaccinia resulting from its transmission through the cow. In this sense, it is clear that variolation after vaccination is revaccination in all excepting in name.

DEATH FROM SUDDEN PRESSURE OF THE TESTICLES.

It appears that among a certain class of people in Germany, on the occasion of resorting to blows, it is customary to endeavour to grasp and squeeze the testicles of one's adversary, and prize-fighters (*querelleurs par profession*) are accustomed to cover and protect these organs accordingly. A few years since, at or near Peitz, a young man, who had interposed between two combatants, was observed to fall on the ground, uttering a loud cry, and, after having been seized with violent convulsions, in a few minutes he died. The surgeon who reports the case attempted in vain for several hours to restore animation; he then left the deceased, the cause of death still remaining in obscurity. But at the autopsy next day the scrotum and penis were observed to be strongly injected and much swollen, though we do not learn that they were afterwards specially examined, and on opening the abdomen about four ounces of blood were found behind the peritoneum, upon the psoas muscle of either side, at the place where the spermatic cord passes through the abdominal ring and the vas deferens leaves the spermatic vessels. At this point the spermatic artery and vein were, if not torn, at least so distended and attenuated that their coats appeared quite porous and cribriform, and had evidently given egress to the blood, which was discovered extravasated.—(*Casper's Wochensc.*, &c.)

THE

DIAGNOSIS, PATHOLOGY, AND TREATMENT

OF

TYPHUS FEVER.

By George Ross, Esq., Surgeon, Enfield.

No. III.

The Petechial Eruption.

My last paper concluded with a few observations on the petechial eruption in typhus; and as much has been said and written within these few years respecting the value of this eruption as a diagnostic sign, I shall now give it some consideration. It has been supposed that this symptom is the only pathognomonic character of typhus, and that it attends this disease as regularly and runs its course as determinately as the eruption of scarlet fever or measles. High authorities—for instance, Chomel and Louis—incline to this opinion, and on this account feel themselves justified in drawing a line of demarcation between typhus and other continued fevers. Dr. Davidson, in his "Thackeray Prize Essay," has taken much pains to class typhus among fevers of the eruptive type on this ground alone. Chomel and Louis state that the petechiæ are renewed about every fourth day; and I think that this circumstance is a stronger evidence even than the fact of the existence of the eruption itself, in support of the opinion of the eruptive character of typhus.

It cannot be denied that petechiæ are a very common symptom of this disease, but the frequency of their appearance is liable to great variety in successive epidemics, and at various periods of the year. During one epidemic, such as that of 1838, the petechial character may almost invariably be displayed; while, during another, it shall be so infrequent as scarcely to merit attention. In all other respects the diseases shall appear so much alike as to defy discrimination. It is possible that an error has arisen on this point, from observing the disease under the peculiar aspect it has shown during a particular epidemic; and also, probably, from indulging a prepossession of the mind, and calling no case typhus but such as exhibited the eruption, or was, by circumstances of proximity or kindred, evidently the effect of the same cause. The authors themselves who advocate this opinion, allow that even in the same family some will show the eruption, and others not. It can scarcely be doubted, therefore, that in numerous families where the eruption has never appeared, and the necessity therefore of naming the disease typhus has not been incumbent, it has been ranged under the head of common continued fever, and thus

presumed to be a disease different from typhus. The idiosyncrasy of the patient or family will also modify the character of the fever. In one family lately under my treatment, the son, who first suffered under the disease, did not throw out the eruption; but the others, seven in number, who clearly received the disease from him, exhibited this symptom. In no other cases out of this house had I observed petechiæ: at the most it can only be said that petechial typhus is a variety of typhus fever.

Though I can by no means admit that the eruption in typhus is an essential character of the disease, yet, when it does appear, we obtain an additional diagnostic symptom, and the peculiar epochs which it observes in its appearances and remissions, serve to elucidate the pathological character of the complaint. This eruption which, in its early stages disappears under the pressure of the finger, fades away, and is replaced by a new crop each fourth day, indicating thereby an intermittent action of the vessels of the cutis, offers a striking point of resemblance between typhus and the regular eruptive fevers.

The Exciting Cause of Typhus.

Having given a view of the pathological character of typhus, both structurally and chemically, and commented upon some of its most prominent symptoms, I shall commence an inquiry into the ultimate causes of the production of the disease. The proximate chemical cause has been largely treated of; the ultimate or exciting causes are those which will now detain our attention. We may consider the cause producing the disease to be the same as that which operates, efficiently, in its propagation; and this position immediately involves the question of its contagious or infectious character. By contagion we understand the communication of a disease by the touch or external surface, by infection, the conveyance of a poison by means of the atmosphere.

There are four modes in which a poison may be supposed to enter the system:—1st, by absorption through the skin; 2nd, by absorption through the lungs; 3rd, by the stomach; 4th, by solution of continuity of the integument, and immediate application of the virus to the blood-vessels and absorbents, and also by direct injection into the blood-vessels. The examination into these several points will determine the mode by which the poison may most commonly affect the system; but this examination will necessarily implicate the consideration of the operating causes themselves; thus, we cannot determine the fact of a patient having received the infection through the lungs, unless we know that he has been exposed to malaria; nor through the stomach unless it can be proved that the necessary cause has been applied. For all practical purposes, therefore, it will be more useful to consider these separate divisions under one head.

The Conditions and Proofs of Contagion.

With regard to the absorption of the poison by the lungs and skin, these questions immediately present themselves,—Is the disease propagated chiefly or wholly by contagion, from individual to individual, by the general influence of malaria, or both? Although much has been said respecting the non-contagious character of one of the most formidable of the diseases of this genus—the plague, yet I am not aware that any weighty authority has deliberately attempted to prove the non-contagious character of typhus. Numerous examples of its contagious character are on record, but it would be merely a work of supererogation to relate them. I have seen numerous examples of typhus by communication, but I shall confine myself to the following, which, besides being interesting on other points, seem to me to carry conclusive evidence as to its communicative character:—A young man, ætat. 21, had been working on a damp, clayey soil, and was brought home ill from typhus. He went through the complaint as usual. *During the progress of the fever* the family, consisting of the father, mother, and seven children, were not affected by the complaint; *but on the supervention of the stage of reaction*, accompanied by profuse perspiration, the mother, ætat. 58, was first seized, and subsequently all the children, with the exception of one who *slept out of the house*, but who came home to his meals, *and partook of the same food* as the rest. The points to be remembered in this instance are these, that the son, first ill, was affected by causes engendered in his own system most probably by some previous atmospheric conditions, for the disease could not be traced to any source of contagion, nor could any evidence of irregularity of diet be obtained; that the family was under an immunity of the disease until perspiration ensued, and then all became affected, with the exception of the father, ætat. 65 (who was, however, at one period in very ill health with febrile symptoms), and a boy who slept from home. That the father did not exhibit fever in a typhoid form may be attributed to his age, which greatly modifies the action of this as well as other animal poisons; but it is a striking evidence in proof of infection, that every other member of the family who slept at home should receive the disease, and that the boy who slept out of the house, but made use of the same diet, should escape. This is, further, a proof that the diet used was not the cause of the disease, but that it was the consequence of infection. Neither, on the other hand, could it have been induced by general atmospheric causes, because not a single case of typhus occurred

in the three adjoining houses, and in which a great number of children was closely packed, and these could scarcely have escaped if the atmosphere had conveyed the poison. The exemption from contagion as regards these depended on the strict exclusion of the infected family, which was carried to such an extreme, that at one time when every individual of the family (with the exception of the father, absent at his work, and the son) were confined to their beds, and unable to help themselves to food or physic, not a single neighbour had benevolence or courage enough to act as nurse. On these accounts there was such a want of cleanliness in the house that the stench was fearful. We have observed that the son who escaped the disease slept out of the house, and this circumstance affords strong corroborative evidence of the opinion that typhus attacks its victims during the night, when the powers of life are at the minimum. It was seen also that the effluvia did not affect the family until the commencement of the stage of reaction in the son first attacked. I have observed this to be the case in all these instances wherein the disease has been clearly attributable to infection. Before the period of convalescence the patient's skin is like a mummy's; there is, indeed, always a certain degree of fetor arising from exhalation from this surface and the lungs, but it bears no comparison with the intolerable odour given out during the profuse perspiration accompanying the reaction. We may consider, therefore, that during the febrile state there is a comparative immunity from infection. If this point were clearly understood and acknowledged, it might save much controversy on the question of contagion or otherwise, since there are many men who, having watched a case of typhus for three or four successive weeks during the febrile stage, and not observing other persons in constant attendance on the patient become infected, have too hastily concluded that the disease is not contagious. It is as well, therefore, to bear this observation in mind. The above was an extreme case, but extreme cases always render the clearest evidence.

The Nature of the Virus considered Physiologically.

Convinced, therefore, by the testimony of facts, that typhus is a contagious disease, and that the virulence of the poison is chiefly manifested during the stage of perspiration, it would be interesting to inquire into the probable nature of the virus thus exhaled. Is it a specific vital secretion, or the product of chemical action?

A secretion may consist either of some substance already existing in the blood, as albumen, or of a new principle formed out of this fluid, either by a healthy or diseased action, as the biliary principle and pus. There can be no question of the contagious character of the pus secreted in eruptive fevers; every inoculation proves it, whether it be the matter of the pustule of the small-pox, or of the bubo of the plague, that be employed. Each of these sources, also, yields matter producing different effects, and these effects are invariable; consequently, the various contagious principles must be inherently different. Chemical or microscopical research may not unfold to us the points of difference, but we are convinced of their existence by their effects. An infectious disease may, however, be communicated by the halitus from the lungs and skin, as well as by direct application of the purulent secretion. Dr. Davidson has maintained that this halitus is nothing but pus in a state of vapour; but pus cannot be secreted except by an inflammatory action, and there is no evidence to prove that this halitus is the result of inflammation. It is probable that in the lungs inflammation may generally exist, but not so of the skin: the idea was ingenious, but it cannot be proved. Neither has experiment proved that fever has ever been produced by exhalation from the skin alone; since the exhalations from the lungs and skin are scarcely separable. We derive, however, a strong presumptive argument in favour of infection being conveyed through the medium of the matter of perspiration from the fact, already noticed, of the virus acting most energetically at the period of the crisis by perspiration. This evidence would tend, therefore, to disprove the assumption of infectious matter being essentially purulent. Moreover, this assumption has been maintained by the force of the collateral evidence derived from the pustular eruption of small-pox; if we also employ the same kind of evidence, derived from the experiments performed with the view of determining the infectious property of scarlatina and measles, we shall find that these diseases have been conveyed by inoculation of the blood of patients suffering under these fevers, consequently infectious matter is not necessarily purulent. That, however, the purulent secretion, when it occurs in febrile diseases, is a mode employed by nature of throwing off the poison, is provable from the state of the urine of eleven cases of small-pox observed by M. Martin Solon. Six of these cases recovered without any effect being produced in the urine by reagents; and there were only four cases of excess of lithate of ammonia, the precipitate in the other case consisting of albumen. It is apparent that the skin was the eliminating organ in the first six cases, and the kidneys in the remaining five; and it contributes to prove that in proportion to the increase of action of the skin there is a decrease of the functions of the kidneys.

The products of secretion from the diseased structures by the skin and kidneys respec-

tively, differ very materially in their qualities, those of the former bearing a closer relation to the proximate principles of the blood itself, those of the latter a more remote. It is questionable if the latter products can communicate disease, but the former communicate it with great virulence. The nearer analogy, therefore, that any secretion exhibits to the principles of the blood, the greater is its contagious power. Pus, consequently, owes none of its virulence to any properties presumed to be acquired during the act of secretion, but is dependent for its activity solely on the qualities of the blood. This conclusion, therefore, involves the principle that effluvia from the human body communicate a disease similar to that from which they originated, with an intensity directly proportionate to their undecomposed state. Hence the reason why all specific pustular diseases are so eminently contagious. There can be no doubt that in all infectious maladies undecomposed animal matter is exhaled by the lungs and skin, and thus extends the influence of disease.

The Nature of the Presumed Malarial Virus.

This argument carries us only so far as concerns contagion, but leads us to no path by which we can form affirmative conclusions respecting malaria. Can malaria produce disease? If it can, what analogy does it bear to the state of the blood of any specific disease so produced? My deductions, as expressed above, declare that the virus of epidemic diseases is specific, and is a product of the changes going on in the living system: if this be true, can decomposing *dead* animal or vegetable matter evolve a principle identical with the vital product? It may be assumed, as a well ascertained fact, that none of the products of putrefaction,—for instance, carbonic acid, carburetted hydrogen, sulphuretted hydrogen,—are capable of inducing that series of changes in the living organism which we denominate fever; but, on the contrary, their effect, either in small or in large doses, is immediate, terminable within a given period, and proportionate to the quantity of the poison inspired; they act like all other known poisons. I have known the contents of an abscess within the abdominal cavity undergo decomposition, within a few days of the first incision being made, and when the fluid, which was in very large quantity, was occasionally let out, the stench was so disgusting that it was scarcely tolerable. Sulphuretted hydrogen was the chief gaseous product. This condition of the fluid continued for a considerable time after the opening of the abscess, yet the patient gradually recovered his health. It may be imagined, however, that although the products of decomposition may not possess the power of causing fever, yet that the emanations from putrefactive matter in the incipient or transition stage may be endowed with this property. This is a plausible notion, but I consider it untenable until it can be proved that putrefactive matter in the incipient stage possesses the same qualities as the virus evolved from the living system. I do not mean to assert that such matter when respired, cannot cause fever, but in such a case I apprehend that there must be some lesion of the lungs by which the matter, is admitted immediately into the circulation to enable its action to become manifest. This, however, can occur only in a few cases; and, on the whole, I should not consider infection from this source, through the lungs, to be a general law of communication.

Evidence that Malaria is not a Cause of Typhus.

Let us now weigh the direct evidence derived from experiment and observation. There is a mass of observations extant tending to prove that emanations from putrefactive sources cause fever of the typhoid type. The last Annual Sanatory Report of the Poor-law Commissioners, which is drawn up with very laudable objects, is replete with such observations; but they are so loosely connected with other existing causes, such as privation and bad food, and are so inconsequential, that they do not offer any points for the foundation of a strict argument. These observations are for the most part mere assumptions and opinions, and have been evidently stated in conformity with the prevailing dogmas on the subject. Fever has been found to prevail in a house with a dung-heap before the door, or in a row of houses whose sewerage is inefficient; consequently the dung-heap or the open sewer caused the fever. Nothing in the way of argument can be more unsatisfactory than this.

But even this report contains evidence diametrically opposed to the conclusions which it is the grand aim of its authors to establish. That typhus or any continued fever does not depend upon malaria is the declared opinion of Dr. Baron Howard, of Manchester, who renders the best digested evidence, and, perhaps, the most important in reference to the locality from which his experience is derived, of all the gentlemen called upon to submit the results of their observations. He says, in relation to the lodging-houses of this town, "The disgraceful state of these lodging-houses has been dwelt upon at some length, because I consider their evils of a most serious and extensive nature, and I feel quite satisfied they are the most malignant *foci* of infectious fevers in Manchester. *Indeed, it is my decided opinion that the vitiation of the atmosphere by the living is much more injurious to the constitution than its impregnation with the effluvia from dead organic matter;* and

certainly all I have observed in Manchester induces me to consider the human miasms generated in overcrowded and ill-ventilated rooms as a far more frequent and efficient cause of fever than the malaria arising from collections of refuse and want of drainage. I have been led to this conclusion from having remarked that fever has generally prevailed more extensively in those houses where the greatest numbers were crowded together, and where ventilation was most deficient, although the streets in which they are situated may be well paved, drained, and tolerably free from filth, than in those where there was less crowding, *notwithstanding their location in the midst of nuisances giving rise to malaria.*" This opinion is strong as to the innocuousness of the malaria from dead organic matter; but there is no ground for the opinion that human miasms can cause fever. It is needless for me to dilate again on this point; carbonic acid, the product of respiration, cannot engender it, and the only resort must necessarily be to the supposition that it is caused by the animal matter evolved. But how can this produce fever until it undergoes decomposition, and becomes subject to the laws of *dead organic matter?* That this is not a probable cause Dr. Baron Howard himself declares. This gentleman, like all the others who have given evidence, has forgotten to consider the wretched circumstances of those individuals constrained by dire necessity to inhabit such loathsome abodes, and the consequent privation and use of bad food which always accompanies extreme misery.

Magendie has endeavoured to prove, experimentally, that the effluvia from putrescent animal matter may cause fever. He confined dogs, pigeons, Indian hogs, and rabbits, over vessels containing such matters; all the dogs died, with the exception of one, which appeared to have been acclimated; the other animals suffered no bad effects. The dogs died in about ten days, apparently of marasmus, and after death there was observed to be a total absence of fat and of chyle in the lacteals, together with inflammation of the mucous membrane of the stomach. Magendie allows that the symptoms and post-mortem appearances were different to those observed when putrid matter has been injected into the veins. They have not, indeed, any resemblance to the conditions in typhus. I suggest that in these cases inanition was the cause of death, and that the appearances in the stomach called inflammatory were caused by the action of the gastric juice; the other animals, also, were not in the least affected. Wherein, then, consists the value of these experiments? In proving that putrescent emanations cannot cause typhus.

The most convincing evidence, however, that has yet been adduced respecting the non-infectious power of animal miasm, is that brought forward by Parent Duchatelet. At Montfancon there is a general reservoir of every kind of filth, and it was presumed that if the effluvia of putrefactive animal matters could induce fever an unusual amount of cases should be found here. This *voirie* comprises 32,800 yards of surface, and receives from 230 to 240 cubic yards of the products of the "fosse d'aisances;" the bodies of upwards of 12,000 horses and of 25,000 small animals are allowed to remain upon its surface to rot. Can imagination conceive any circumstances that could possibly exhale a more abundant effluvium, and more highly calculated to excite febrile disorders? The stench is described as being of the most repulsive and intolerable character. It is, at all times, utterly insufferable within a circumference of two thousand yards, and sometimes extends, in all its intensity, over a circumference of 4000 yards; nay, particular winds have been known to carry it to a distance of eight miles! I shall not enter minutely into the different processes which this filth undergoes for the purposes of trade, and the consequent necessity of the men engaged on the works to be constantly in the midst of those disgusting odours; nor shall I give any account of the aggravating circumstances; it is sufficient to know that the *voirie* of Montfancon contains a concentration of every sickening and disgusting putrefactive odour. What, then, is the health of the persons inhabiting this quarter? Are they wan, haggard, and emaciated, subject to fevers and premature decay? Not at all. They are a healthy, vigorous set of people, and look like butchers; many of them attain great longevity, and typhus fever seldom occurs: indeed, they regard these emanations as safeguards against contagious diseases. This is not only the case with those who are acclimated, but even fresh workmen, who arrive daily, suffer no inconvenience after the first few weeks. However improbable this statement may seem, it stands upon high authority, and is well authenticated. Parent Duchatelet also states that two hundred exhumations are annually performed in one of the cemeteries of Paris, and although the bodies are removed in every stage of decay, yet no case of typhus has been known to occur in consequence. This is overpowering evidence, to which trivial and isolated observations must succumb.

Edinburgh is famous for the production of typhus fever, and it is said that the land east and west of the city, covering an extent of three miles, is matured by fetid irrigation. These have been long regarded as cause and effect, yet there has been so little coincidence between the periods of the virulence of the fever and the increased amount of the fetid exhalations, that Dr. Alison, after an elaborate inquiry, concludes that "the true spe-

cific cause of the contagious fever, at least in Edinburgh, certainly does not spring from anything external to the living body."

The Effect of the Injection of Putrid Matter into the Veins.

Although, therefore, it is clearly proved that the emanations from putrefactive matter cannot affect the system through healthy lungs, yet it by no means follows that this matter cannot in any way operate on the system. M. Gaspard experimented on a dog by injecting fermented cabbage into the jugular vein: the animal died on the fifth day. He presented before death all the symptoms of typhus, and the appearances, post mortem, precisely corresponded with the symptoms. The duodenum, rectum, and a portion of the small intestines, were discovered to be inflamed; the mucous glands of the rectum were swollen and very distinct; the mesenteric glands engorged with blood and inflamed; and black, thickened, ropy bile was found in the gall-bladder. This single experiment clearly manifests the production of typhus by injection into the veins of putrid matter, and declares, also, that the specific action of such virus is expended on the glandular organs of the abdomen.

Putrid Matter taken as Food a Cause of Typhus.

I now approach to a branch of my subject hitherto new, so far as concerns direct observation and experiment, among the causes usually assigned as productive of typhus. Physiologists have long been searching after the causes of typhus in the surrounding atmosphere, but have scarcely once supposed that such causes could exist in the food taken into the stomach. Like the dog in the fable, they have dropped their meat in diving after its shadow; they have neglected to inquire into the effects of putrid matter taken as food, but have very zealously sought after the presumed influence of its emanations. Privation has been commonly assigned, of late, as the chief cause of typhus, and so it is in an indirect way, because privation and bad food are invariable associates. Dysentery and typhus fever, we are told, are the constant attendants on a retreating army, and those individuals who are at all acquainted with military statistics are aware that food, in a more or less putrid state, is often taken, by necessity, during a harassing march. It requires no prophet to convince us of this. However, as I carefully repudiate all general observations, I shall adduce a few examples to prove the influence of bad food in the production of typhus. A remarkable instance of this kind came under my notice a few years since, but before I give it in detail I shall quote from the last number of the "British and Foreign Quarterly Review" an account of a disease produced by eating bad food, which was observed by Dr. Sigg at Andelfingen.

Between five hundred and six hundred persons assembled at a musical festival, held in the church of Andelfingen, and, after having spent four hours in the church, they sat down, in an outhouse, to a dinner of cold veal, ham, and salad, with indifferent wine and beer. "The meat did not look good, and the ham had such a strong taste that many persons did not partake of it, though most made a hearty meal." Four hundred and forty-four out of six hundred persons were attacked by disease between the tenth (the day on which the food was taken) and the 20th of June, of whom nine died with typhoid symptoms. Many on their way home vomited, and suffered afterwards but little inconvenience; others were attacked between the fifth and tenth day with nausea, vomiting, and diarrhœa, pains in the limbs and head, slight rigor, loss of appetite, great thirst, and an extremely unpleasant coppery taste in the mouth. About the seventh day (of the fever, I presume), the *epigastric* region became extremely tender, and the *bowels were constipated;* occasionally, however, diarrhœa came on at this period, and the stools were dark green and highly offensive. There are no data made known by which we can determine whether the cases that presented diarrhœa as a prominent symptom ran into the typhoid state, or received benefit from this symptom, and recovered. I have already expressed my opinion on this point in respect of the ordinary cases of typhus. *Delirium* accompanied this stage, and the typhoid symptoms became strongly marked. On the twelfth day a state of torpor supervened, and the typhus was more confirmed. Between the fourteenth and seventeenth day slight cough, with expectoration, ensued, and the patient usually became convalescent. If, however, the case was to terminate fatally, the typhoid symptoms increased, the abdomen became tympanitic, the stools horribly offensive, and often mixed with blood; great weakness and fainting ensued, and death closed the scene.

The post-mortem appearances of four cases exhibited—congestion of the membranes of the brain, sero-sanguineous infiltration of the lungs, some redness on the lower part of the external coat of the ileum; which, on being laid open, showed on the mucous membrane various dark red-coloured patches, very friable, and if the disease were farther advanced presented patches of ulceration, varying in size from that of a mustard-seed to a fourpenny-piece.

There can be no question that (with the exception of some cases of a dysenteric character) these were veritable cases of typhus fever. The symptoms, and more particularly the post-mortem evidence, announce

and confirm it without a doubt. These cases carry a witness with them as strongly confirmative of the production of typhus by the use of bad food as does the *voirie* of Montfancon of the non-infectious nature of animal effluvia. My own experience is corroborative of the same point.

A few years since the following facts fell under my observation:—Two hundred persons consumed meat (that had been, for the most part, only partially cooked, and was still putrescent), during two or three days. On the fourth and few successive days after the last employment of this food, twenty-five of them were seized with fever, of whom seven suffered under scarlet fever, four under typhus, and fourteen under common continued fever. Those only have been called cases of typhus that were attended by delirium, and betrayed the customary typhoid symptoms; but I do not believe that there was any essential difference between the common continued and the typhoid fevers. One of the cases of typhus was followed by numerous abscesses during the convalescence, as occasionally happens after a fever of great severity. None of the patients died, consequently pathology can throw no light upon the nature of the disease. There could be no doubt, however, of the character of the diseases from a consideration of the symptoms; neither could it be disputed that these fevers originated in the cause assigned, since they all happened within one or two days of each other, and thus could not have been conveyed by infection, or any other progressive means. Moreover, there was not a single other case in the neighbourhood for some miles round. These examples amply prove that typhus may be induced in the system by the use of putrid food. In addition to the foregoing I have traced various other cases of fever to the employment of bad diet, but I shall not enumerate them, as less important facts cannot add much weight to the testimony afforded by observations on so extensive a scale. That putrid food is a very frequent cause of typhus I am convinced, and it is probably the usual one of all sporadic cases. It is a cause, indeed, that exists less among the rich than among the poor, and among the latter there is often a great jealousy of acknowledging the consumption of bad food. There are some, however, who are not ashamed of their poverty, and do not resent a particular inquiry into their habits of living, and from them the truth is most easily ascertained. By pursuing our inquiries in this direction we have something evident and tangible before our view, something recognisable by the senses, and whose qualities and effects can be easily obtained and recorded. This is not the case in our investigations of the miasmatic state of the atmosphere; here it is all an airy nothingness, which constantly tempts and always eludes our grasp.

Experiment to prove the Effect of Putrid Animal Matter taken into the Stomach.

To satisfy myself that putrid matter taken into the stomach could, undoubtedly, produce typhus, I commenced a set of experiments, of which, however, I have found the opportunity as yet of completing one alone. I fed a rabbit, being the animal, of all others, in which vomiting is least easily excited, on putrid lung for two days, giving him a little each night and morning, with a sufficiency of water, and a small quantity of bran. No particular symptoms seemed to have been induced beyond unusual sluggishness, and there was no diarrhœa, yet the animal died within thirty-six hours after the first meal.

Post-mortem.—The stomach was found full of semi-digested matter, which, on being removed, was covered with a coat of chymous matter, or probably epithelium, which had disappeared from the surface in many places, more especially towards the fundus. There was no redness of this organ. The jejunum was healthy, though probably slightly injected. The ileum, however, was diseased on the internal surface over its whole extent; its glands were softened and enlarged, and of a greyish-white hue; the mucous membrane intervening between them was highly injected, and of a somewhat livid red colour; it was covered with pus. The absorbents in the adjacent parts were filled with pus. Two or three mesenteric glands, cut through near the junction of the ileum and cœcum, were of a purplish-red colour, soft in the centre, with a wall, of a white colour, about a line in thickness; when these were pressed with a scalpel a quantity of pus exuded. Another gland was also of a purplish-red colour, had not a white wall, and did not contain pus; it was firm, and a white vessel was seen running through it. This gland was merely in a state of inflammation. The gall-bladder was filled with a thickened greenish black bile. The colon was distended with fæcal matter, which did not give out a fetid odour.

These are the precise pathological characters of typhus, and there can be no question of the genuine reality of the disease induced. The evidence brought forward in this paper is amply sufficient to suspend a prejudiced decision, to incite the reflective mind to inquire into the truth or error of all its old-established opinions, and to examine the value of the evidence upon which they were established. Authority sanctifies an error because we are by nature prone to reverence the teachers of a past age, believing them, therefore, to be wise, and because men are commonly too indolent to ask questions and answer them for themselves.

Analogy between Typhus and Scarlet Fever.

It cannot have escaped the observation of

some of my readers, that scarlet fever was enumerated as one of the diseases induced by the consumption of putrid food. This is a fact, and each individual can judge for himself as to its value; for my own part, I am firmly of opinion that scarlet fever and typhus are always excited by the same causes, and acquire their difference of development from some constitutional peculiarity or state of atmosphere. I may remark, that I have invariably observed typhus and scarlet fever, when epidemic, to prevail together, or within a very short period of each other, scarlet fever usually succeeding to typhus. This does not happen casually, nor is scarlet fever incidental to typhus, as measles or chicken-pock may be; but they often occur together in equal abundance, and make it doubtful whether the one or the other be the prevailing epidemic. This concurrence happens generally in the autumnal months, when typhus acquires more especially the character of an epidemic.

The probable Mode of Action and Composition of the Virus.

There is now but one essential point left for our consideration relating to the production of fever, namely, the intimate nature of the morbific material, the *materies morbi* of typhus. Liebig has acutely supposed that the virus must act as a ferment, and create in the ultimate molecules of the system the same kind of action as exists in itself. This conjecture is, probably, virtually true, for how otherwise can so small a quantity of the lymph of the variolous eruption as is scarcely perceptible communicate so energetic a disease to the system! It may be asked in what way can putrid matter, taken into the stomach cause fever, since the gastric juice is a powerful antiseptic? It is upon this fact, announced by John Hunter, that I expect the greater number of objections to my doctrine to be based. The fact I admit; but there are some cases in which there is a deficient secretion of gastric juice; others in which a much larger portion of putrid matter may be consumed than the usual amount of gastric juice secreted can possibly correct; and, above all, the fact that disease may be induced through food taken into the stomach stands upon direct and incontrovertible evidence. It is, in truth, upon this very fact of the assimilating power of the gastric juice that I explain the phenomena observed. By its means the putrescent matters undergo a partial assimilation, sufficient to confer on them that amount of change necessary for absorption and combination with the organic molecules, but not sufficient to deprive them of their poisonous properties. Hence the production of disease in general, and, in particular, of typhus fever. Thus much concerns the mode of action of the virus. It is worthy of observation, that when it is necessary to communicate an eruptive disease by inoculation, it is desirable, for the assurance of success, to employ the earliest lymph that is secreted, and to insert it while yet recent. This lymph, whether contained in the pock or between glasses, becomes decomposed by exposure to the oxygen of the atmospheric air. When the lymph, for example, of the vaccine vesicle has been kept for some days between glasses it will often assume a prismatic appearance, which appears to me to result from the formation of water. When this happens the lymph is inoperative. From this circumstance I should conceive that the elements causing febrile disease have a strong affinity for oxygen, and are, perhaps, chiefly carbon and hydrogen in a peculiar state of combination; the subject is, however, so obscure that we are scarcely warranted in hazarding conjectures. The rapid decomposition of the vaccine lymph by oxygen should not, nevertheless, be forgotten in respect of its therapeutic indications.

The foregoing observations on the causes of typhus have expanded under my hand into greater bulk than I at first conceived that they would occupy, and have made it necessary for me to require the attention of the reader to an additional paper. I could not, however, contract my remarks either in justice to the subject I had undertaken to treat, the importance it holds in the investigations of medical philosophers, or to the views which I have endeavoured to establish.

THE SEAT OF THE SOUL DISCOVERED.—The Chinese say that before performing inoculation of the small-pox a lucky day should be chosen for the operation. The eleventh day of the moon ought to be avoided, for at that time a person's spirit (or spiritual essence) is in the pillar or *septum of the nose*; also the fifteenth day of the moon is bad, because on that day the spirit is in the sides of the body!—*Dublin Journal of Medical Science.*

IODINE INJECTIONS INTO CHRONIC ABSCESSES OF JOINTS.

M. BONNET, principal surgeon of the Hôtel Dieu at Lyons, states that he has derived the most favourable results from the employment of injections of iodine into indolent abscesses unattended by puffiness (*gouflement*) of the cellular tissue, or thickening of the synovial membrane, and occurring coincidently with emaciation, &c., in children of a scrofulous habit, in whom the powers of the constitution are much enfeebled. In no case has he observed the alcohol of the tincture of iodine employed to produce inflammation in any formidable degree; but his experience of the remedy, as thus adopted, has certainly been too limited hitherto for the establishment of any decided opinion respecting the utility of the remedy against scrofulous abscess.

PHYSIOLOGY OF DISEASE.

ITS DIVISIONS.

THE HUMORAL PATHOLOGY.

To the Editor of THE LANCET.

SIR,—As I perceive that I have been anticipated in the application of the theory of eremacausis to the phenomena of contagious and infectious diseases by more than one of your correspondents, I shall take a wider field in my present communication, making the general theory of disease my subject. It seems to me of the highest importance that our general notions should be correct, since although they, themselves, are deduced from a multitude of particulars, yet, as most men have not the time to go, or will not take the trouble of going, through that process, these general notions must be communicated to them by those who *will* do so. Though our general notions are thus originated, yet the practice of medicine depends on the applications of these generalities to particular cases. I shall first make a few observations on the humoral pathology, then on the classification of diseases, and lastly, apply the humoral pathology to the explanation of one of the primary classes, leaving for a future opportunity the consideration of the remainder.

The bodies of men and animals were, from the first origin of medicine, seen to consist of two parts: fluids and solids. It was natural that diseases should be referred to one or other of these chief constituents. At the same time it was observed that the loss of the chief fluid of the body caused immediate death; or, if not sufficient in quantity to produce that effect, gave rise to great debility, and frequently laid the foundation of future disease. Thence arose the dogma of the Mosaic law that the blood is the life. On these circumstances the humoral theory was founded, a theory which, with various modifications, ruled the medical world from the time of Hippocrates to that of Cullen, who completed the revolution from fluidism to solidism, for which some slight preparation had been previously made. He ran from one extreme to the other; the older physicians had attributed all the ills which flesh is heir to, to the fluids; he, on the other hand, to the solids.

It is, at first sight, astonishing how little opposition this entire change of opinion met with on its first promulgation, and how greedily the new theory was caught at by the theoretical physicians of the day. In most cases any considerable alteration of opinion is opposed with great hostility, and a long time must elapse before it becomes generally established, *e. g.*, Harvey's discovery of the circulation of the blood. Here, however, so many gratuitous hypotheses had been added to the original theory; there had been so little observation of phenomena and regular induction from them, that any set of opinions *apparently* placed on a firmer basis found almost immediate approval. Solidism, however, has, in great part, the merit of being the parent of pathological anatomy. Solidism has held its influence over the minds of medical theorists almost to the present day. It is, however, impossible that a fluid which plays so important a part in the functions of the body should remain long neglected. Changes, first in its physical, then in its chemical characters, were observed, and with the progress of organic chemistry these changes have been more and more accurately investigated. A vast field of inquiry thus opens on our view, pregnant with the most important results; for supposing many diseases to depend (as most certainly they do) on changes of the blood, which changes, in their turn, are indicated by certain symptoms, from which their nature may be inferred, may not both the indications and mode of treatment be deduced with far greater certainty?

The above general observations being premised, I now proceed to the general classification of disease which appears to me most natural and accordant with our present knowledge. I take that which was used by Dr. Elliotson in his lectures at University College.

According to this arrangement, diseases are divided into three classes:—

Universal.—Affecting the whole system at the same time without evidence of local disease.

General.—Affecting some part of the body, but capable of occurring in any of the tissues.

Local.—Arising from mechanical, physical, or chemical causes, or from change of nutrition in a part dependent on vital action.

The preceding characters of the classes are, I am aware, imperfect, although they express as nearly as possible my ideas on the subject. We must not forget that an intimate connection is established between the three classes, since each may be the exciting cause of the other, for universal may produce general or local diseases, and *vice versa*. Nor can we be surprised at this, for if we look around us we shall find all natural objects more or less closely connected, and that the whole circle of sciences is inextricably interwoven.

Under the class of universal diseases, or those that affect the whole body, are included fevers of all types, whether contagious or non-contagious. These are very variable in their character, and are frequently accompanied by general diseases; so frequently, that fever has been considered the result of general diseases, rather than their cause.

We can, upon the solidists' theory, have no clear explanation of the cause of fever; for we find that the solidists themselves are divided in opinion. In general they believe

that fever is the result of local inflammation, and one attributes it to inflammation of the brain, another to inflammation of the lungs, a third to gastro-enteritis, a fourth to inflammation of the liver, and others to similar affections of other important organs; so that inflammation of one or other of the more important viscera has been alternately accused of being the cause of this ubiquitous disease. This very diversity of opinion on the same subject shows that the whole theory with regard to this disease is by no means soundly based.

But are there no cases of fever which exhibit no symptoms of local inflammation during life, or appearances after death? There is no doubt that such cases occur, for we frequently meet with them in practice, where no symptoms of local inflammation can be observed; but they are usually slight cases, and unfrequently fatal. It is evident that when any great excitement or rapid flow of blood through the vessels occurs, local congestion and subsequent inflammation are probable results, and as the vital force in different organs in different subjects is unequal, we may expect that that organ which is most deficient in vital force will become affected. Hence we find traces of inflammation in different organs in different cases.

Infectious fevers, and particularly exanthemata, usually classed very absurdly with skin diseases, afford the most striking evidence that universal diseases are produced by morbid alterations of the blood. Take a case of typhus, for example, and we find it is dependent either on decomposing animal or vegetable substances, causing impurity of the air, or on morbid emanations from a person suffering from the same disease. This external morbific matter must enter the system either by the skin or mucous surfaces. These surfaces have the power of absorption; they convey it into the blood, with which it circulates, and gives rise to the production of a similar morbific matter, which finds its exit at the surfaces of the body, and produces at the same time the alterations of the solids which are seen as the peculiar symptoms of the disease. Here we have a simple explanation of the phenomena of typhus. But it may be objected that the morbific matter which produces the disease may act primarily on the nerves rather than the blood. This objection may be answered by reference to exanthematous fevers, particularly variola, where the morbific matter is both visible and tangible. A minute portion of small-pox matter is introduced directly into the blood-vessels by inoculation, it circulates with the blood, and, after a longer or shorter period of incubation, the symptoms of fever make their appearance; and when the disease arises from simple contagion or infection, we can only guess that variola is about to occur until the eruption shows itself. Now, here we have direct introduction of the morbific matter into the blood, and a fever produced by the introduction of that matter; the alterations in the solid parts being the effect, not the cause. Again, if we inject putrid animal fluids into the veins, we produce a fever resembling typhus. Thus it is proved that *fever is the result of alterations in the circulating fluid.*

It is in these cases that the theory of eremacausis is of such great value. In a paper read some two or three years since at the University College Medical Society, I used precisely the same train of argument; but a difficulty presented itself at that time which appeared to me inexplicable. It was this: how, in a case of small-pox, is so profuse a quantity of morbific matter produced by the introduction of so minute a portion into the circulation? The law that substances in a state of eremacausis, or decay, when placed in contact with a proper fluid give rise to the formation of similar matter by exciting a species of fermentation, affords us a clear explanation of the phenomena of small-pox. Thus the introduction of the minute portion of variolous matter into the blood produces a molecular motion (or fermentation) by which some of the elements of the blood arrange themselves into the form of variolous pus. We are not at present aware which of the constituents of the blood undergoes this change; but it is by no means improbable that the fibrine is so altered, as the coagulum is extremely loose and tender in this disease.

Moreover, the progress of the symptoms of variola appears to afford a certain proof of the affection of the blood; for we find no visible effect produced by the introduction of variolous matter into the blood for some days; whereas, when any external cause operates on the nervous system, its effects are almost instantaneous; but in this case an interval of several days occurs between the application of the cause and the appearance of the effects. Great general excitement and the common symptoms of fever follow, the only peculiarity to be observed being the excessive pain in the back, a symptom which would not, without some collateral circumstances, enable us to determine the nature of the disease.

We surely cannot rationally explain these symptoms by attributing them to affection of the nervous system; and there is no other set of organs to which the solidist can fly, for no others have so general an operation on the system. To the blood, then, we must refer this disease, and in so doing all the symptoms are easily explicable.

In the first place we may then infer, that the minute proportion of variolous matter entering the blood sets up the species of fermentation noticed by Liebig; but as the ferment is at first minute in quantity, it increases so slowly that for some days it produces no appreciable effect on the general

health, until, having arrived at a certain proportion to the whole mass of the blood, it begins to react on the nervous system, and produces the unpleasant sensations that usher in an attack of fever. With the continued increase of the variolous matter the symptoms become more severe, the fever rises to a greater and greater height, until it becomes necessary that the morbific cause should be expelled. Now, some unknown properties of variolous matter determine its expulsion by the skin in the pustular form. We are ignorant why variolous matter should be expelled under this form rather than any other; but although we are incapable of explaining it, the theory of the affection of the blood in this disease is not weakened by our ignorance. The morbific cause is thus expelled, and in proportion as the blood is purified health is gradually restored. But, in severe cases, where the pustules are confluent, the evacuation of their contents leaves an ulcerated surface coequal with that of the body. The action of the air on this surface, the reaction of so large a diseased surface on the nervous system, and the contact with the absorbent ulcerated surface of a mass of putrefying matter, produce the secondary irritative fever, of which so many patients die. In the foregoing sketch I have not adverted to the increased secretion in the fauces, and other minor symptoms, which may be explained on the same principles.

I know the objection may be raised that variola and several other contagious diseases usually occur but once in the life of an individual, and that we cannot suppose a permanent change in the nature of the blood which should render the second application of these morbid poisons ineffectual. This is certainly a forcible objection, and one that it is not easy to answer on any theory. I shall, therefore, content myself with showing that if it cannot be explained on the humoral theory, it is equally inexplicable on any other. The solidist must of necessity refer the action of morbid poisons to the changes induced by them on the nervous system, and these changes must affect either the function or organism of that system. But the change in the organism or function which prevents a second attack of variola must be permanent so long as indifference to the action of the morbid poison exists. If the changes of the nervous system producing this indifference be so slight as to be perceptible to us only in that indifference, may not, I ask, the changes induced in the blood be equally slight and imperceptible, except in the result? It is useless to aver, in defence of their principles, that the blood is a fluid in a state of constant motion and change, because I may retaliate by saying that the nervous system, and indeed the whole body, is universally allowed to be in an equally constant state of motion and change, and thus that what is true of the one is equally true of the other.

In my previous remarks I have confined myself to fevers and exanthemata, but I might have cited several other universal diseases in which an abnormal state of the blood is still more evident, *e. g.*, anemia and scurvy.

I shall close this letter on universal diseases with the discussion of the phenomena of intermittent and remittent fevers, to which the humoral pathology will be found more applicable than that of Cullen (which is, I imagine, almost fallen into oblivion), as giving a more consistent account of the phenomena. Ague presents a striking resemblance to typhus, so far as regards its cause, both being originally produced by the emanations from decaying substances, but differing in this respect, that the former is neither contagious nor infectious, whereas the latter is eminently so. A person is exposed to malaria, and shortly after suffers from an attack of intermittent or remittent fever, which returns, or is aggravated at regular intervals; each accession is characterised by three stages, cold, hot, and sweating, after which the symptoms wholly disappear in the intermittent, and become much slighter in the remittent type. I may here remark that the remittent type is an intermediate form which connects intermittent with continued fever. The cause, like that of typhus, is external, material, and produced during the putrefaction of vegetable substances; it is, in short, a substance in a state of decay; and if we may credit the report of a French physician, whose remarks I remember to have read, although I cannot now quote my authority, it may be condensed by cold, and appears in the form of a slimy substance, nearly approximating in its characters to animal matter. That this is not surprising may be gathered from Liebig's work, in which the close resemblance of vegetable albumen, fibrine, and caseine, to that of animal origin is shown. So far the arguments I have used in speaking of typhus and variola are applicable. But how explain its periodicity? Here we must call in to our assistance a general law of the animal economy which has been so ably demonstrated by my talented friend, Dr. Laycock, and which is by no means incompatible with the humoral theory. This law of periodicity obtaining, as it does, not only in the abnormal, but also in the normal states of the system, has an undoubted influence on all diseases, although we are not thereby warranted in considering all diseases as modifications of ague, and treating them as such. It is not, then, inconsistent to consider that ague arises from a morbid change of the blood, the manifestations of which are regulated by the law of periodicity. It is abundantly evident that in the present state of our knowledge we are not admitted behind the scenes to ex-

amine every wheel of the machinery that sets them in motion, but this should not discourage us from the endeavour to solve the problems of medical science. I may be wrong in many of the opinions I advocate, but I now call upon the advocates of solidism to give a more rational theory of these universal diseases.

Before closing my letter I wish to solicit the attention of the medical world to the two following problems, the solution of which will, in my opinion, give a greater certainty to medicine than it has ever yet acquired. They are to my mind of the greatest possible importance, and I would gladly undertake them were I placed in favourable circumstances for the investigation, however laborious it may be. These are, first, to ascertain what are the chemical changes of the blood in various diseases, and what connection they have with the symptoms of the disease? Something has been done in this matter, but much remains to be done, and it is only by the comparison of the state of the blood in numerous cases that we shall arrive at any degree of certainty. What, for instance, are the changes in quantity and quality of the constituents of the blood in typhus, variola, &c., and what new substances are superadded? The second problem has reference to the action of the more important remedies on the blood; what chemical and physical changes they produce? That they do produce such changes there can be no doubt, since most of them are absorbed before their effects are manifested. What, for example, is the alteration of the blood produced by mercury; on what particular constituents does it act; and how does it modify them? Many opportunities of investigating these changes in the human subject occur in our hospitals without injury to the patients; but, at all events, we are justified in conducting such experiments on the lower animals. The only drawback is the expense; for large animals must be used for this purpose, since the mere loss of a pound of blood to ascertain its normal condition would be productive of great alteration of this fluid in small ones.

I shall take a future opportunity to enter on the subject of general and local diseases; the former of which (inflammation, and other diseases that occur in any of the tissues) I believe to be dependent on disease of the blood, the latter on mechanical, chemical, or physical causes; on the effects of universal or general diseases on particular structures; or, lastly, on alterations of the vitality of particular organs or systems. I am, Sir, your obedient servant,

PHILIP B. AYRES, M.D.

Thame, Feb. 22, 1843.

THE pectineus muscle sooner becomes putrefied, in general, than any other muscle in the body.—*Dr. Knox.*

METASTASIS.

PHLEGMASIA DOLENS AND RHEUMATISM CONSEQUENT UPON OTHER DISEASES.

MM. TESSIER and RECAMIER consider the following cases, which were treated at the Hôtel Dieu, Paris, as well-marked examples of metastatic action.

A woman, in the period of lactation, experienced after a meal, during which she had felt much oppressed by heat, a sudden and total suppression of milk. Symptoms of meningitis soon displayed themselves, for which she was brought to the Hôtel Dieu. The inflammation was subdued by active measures, but the patient was soon afterwards attacked afresh by fever, accompanied with cough and expectoration, and all the symptoms of pneumonia. This, in its turn, gave way to a mild peritonitis, and this again to phlegmasia dolens, from which the patient recovered by a lingering convalescence.

In another case, cited by the same authorities, the round of affections commenced with phlegmasia dolens. The diseased action passed from the right leg into the left, where it assumed the form of neuralgia. It thence gradually proceeded along the crural nerve and to the abdomen, where it was combated by frequent purgatives.

A case of pneumonia, in its first stage, was treated in the Hôtel Dieu, by ordinary antiphlogistic measures. It speedily yielded to these; but soon a sharp attack of arthritis, with a good deal of inflammatory swelling, took place in the knee. In a few days this disappeared, being substituted by pain in the region of the heart, and all the symptoms of pericarditis. A large blister was placed over the precordial region, and sinapisms were applied to the knee; in a few days the severity of the disease subsided.

The metastasis in this case was remarkable. Its progress was one that not often occurs in man, but is conformable with the march of the disease in lower animals. In the horse, a rheumatic affection of the joints is commonly preceded by a pleurisy; in the human subject, the articular serous membranes are usually attacked before those of the great cavities. Another remarkable circumstance was the speedy removal of the pneumonia, a tendency to sudden resolution being common to rheumatic inflammations.—*Gazette des Hopitaux*, Jan. 21.

SOME attribute great influence to the nervous system in coagulation, and have said that in destruction of the nervous centres, lightning, poisoning by narcotics, prussic acid, death from mental emotion, &c., the blood does not coagulate. The blood perhaps remains longer fluid in these states, but it *does coagulate*. Previous results are either in error or may be explained away.—*Dr. Sharpey.*

HEPATISATION OF THE LEFT LUNG, AND DISEASE IN THE RIGHT SIDE OF THE HEART,

PRODUCED BY A FIT OF INTOXICATION.

To the Editor of THE LANCET.

SIR,—The insertion of the following case in your valuable Journal will oblige your's obediently,

M. D. THOMPSON, M.R.C.S.L.

Stalybridge, Feb. 23, 1843.

The subject of this case had been employed in the card-room of a cotton factory for the previous fourteen years. About two years ago he had pericarditis and anasarca, from which he apparently wholly recovered in a few weeks, by depletion and a course of mercury. During the remainder of his life he had enjoyed good health until the 14th inst., when about three o'clock, p.m., he was visited by my intelligent pupil, Mr. Stopwood, who thus describes the condition in which he then found him :—

"On visiting J. W. on the 14th of February, I found him a strong-built man, of the bilious temperament. His countenance manifested considerable distress; his breathing was natural; circulation weak, but regular; pulse about 60 beats per minute, soft, and compressible; the skin was moist, but rather below the natural heat; the vessels of the membranæ conjunctivæ were injected with red blood; but both he and his wife declared that such was their ordinary condition. He complained of pain over the whole of his body, but particularly in the extremities, which he stated himself to be nearly incapable of moving. He had had for many hours almost incessant vomiting, and his bed was saturated with the ejected matters; his stomach, he said, felt 'quite sore,' which he attributed to his efforts to vomit. Upon questioning him concerning the commencement of his illness, he confessed to having drunk a pint of strong rum about twenty-four hours previous to my seeing him, to which he imputed his ailment. He had voided a considerable quantity of urine since the occurrence, but had had no evacuation from his bowels."

On visiting him at ten o'clock, p.m., of the same day he was restless, and complained of an acute pain opposite to the apex of the heart, increasing on respiration, and also considerable pain along the left side of his body, especially in the left arm and the left leg. These limbs he stated to be nearly powerless, an effect supposed to have been occasioned by having lain for several hours upon them, in the cold, while under the influence of the rum which he had previously drunk. His countenance was anxious and expressive of great physical distress, his tongue tremulous and coated, and his pulse slow and feeble.

On percussion, a greater extent than natural of the precordial region emitted a dull sound, and on placing the ear to the same part no impulse of the heart could be discerned, but a distant *rustling* or *friction-murmur* was audible.

On the 15th he was afflicted by a loud tracheal rhonchus and suffocating dyspnœa, no pulse was distinguishable at either wrist, and he died in the course of the following night.

Suffice it to say, that the treatment of this case consisted of bleeding, calomel and opium, blisters, and sinapisms.

Sectio Cadaveris.—On the chest being opened the left lung was found compactly filling all the left side of the thorax; it was very hard, in fact, completely hepatised, with the exception of a small portion of its upper lobe, its sections yielding, on being firmly pressed, a considerable quantity of a dark-red grumous fluid and a greenish-white muco-purulent matter. The latter filled the bronchial tubes, the internal tunic of which had a dark reddish-brown appearance. The right lung was apparently healthy. The left pleuritic cavity contained a quantity of serum. The diaphragm had a considerable convexity upwards, so much so as to elevate greatly the apex of the heart. The pericardium contained from one to two ounces of turbid serum, with some flakes of lymph, and the apex of the heart adhered to the pericardium by a small membranous band. There were some deposits of lymph, of about the magnitude of a sixpence, but of an oval figure, between the external serous and the muscular tunics of the heart. In the right auricle and ventricle a thick white elastic substance, apparently organised fibrin, was discovered adhering in the ventricle so intimately to the columnæ carneæ that it could not be removed thence without laceration of the latter; it also extended along the tube of the pulmonary artery, nearly obliterating its calibre. The left ventricle was considerably hypertrophied.

On the abdomen being opened the bowels were found distended with wind, but no further indication of disease manifested itself amongst the viscera of this cavity. The head was not examined.

It appears to me probable that the predisposing cause of disease in this case was extreme nervous exhaustion, so extreme as to approximate closely to death, and accordingly to occasion a diminished quantity of blood in the left side of the heart and the *arteries*,* with a proportionate increase of

* As it is doubted whether or not the arteries possess an independent power of contraction, I beg to state that lately, in one instance, after having firmly tied two ligatures, two inches from each other, on a

blood in the right side of the heart and the *veins*, in consequence of the former possessing greater irritability and contractility than the latter. I also consider the exciting causes to have been gravitation and cold, occasioning the condition of the lung termed by Dr. Williams *hypostatic*, and that the proximate endocarditis, pericarditis, and *pneumonia agonisantium* (as thus termed by Laennec), were the effect of the struggle occasioned by the distention of the right side of the heart, and the obstruction of the right pulmonary artery, arising from its engorged capillaries.

The practical inference deducible from the above case is, I think, that no individual in a state of intoxication should be permitted to lie in any single posture for a long period without intermission, or in a cold place.

FUNCTIONS OF THE LYMPHATIC SYSTEM.

To the Editor of THE LANCET.

SIR,—Dr. Carpenter has written to me a note in which he claims priority in the declaration of the new functions assigned to the lymphatic system, and has forwarded me several pages of the proof-sheets of his "Principles of Human Physiology," published twelve months since, which contain the enunciation of this doctrine.

I have already privately assured this gentleman that I had never seen his work, and had not been made acquainted with his views in any manner, so that my originality is not affected.

Dr. Carpenter has freely and clearly stated the principle in question, and thus far I recognise his claim and give him credit for philosophical acuteness. He draws my attention to the following passage, as declaratory of the general principle:—

"It may be stated, then, as a general fact, that the function of the absorbent system is to take up and to convey into the circulating apparatus such substances as are capable of appropriation to the nutritive process, whether these substances be furnished by the external world *or be derived from the disintegration of the organism itself.*"

The words printed in italics show that Dr. Carpenter believed that the waste matter of the system was capable of being reconverted into nutritive matter; a position, in my opinion, inconsistent with the laws of physiology, and altogether insusceptible of proof. I have avoided this view of the question on this account.

My sole principle is, that the unorganised adipose matter receives the necessary changes, fitting it for organisation, in the lymphatic vessels. It is true that Dr. Carpenter recognises the probability of the transformation of oily matter into albumen by the agency of the blood in the lactiferous system; and so also did Müller, although this physiologist failed to observe that which was apparent to Dr. Carpeater, namely, the analogy between the two systems of vessels. The principle, as announced by Dr. Carpenter, rests entirely upon this analogy. The principle, as contended for by me, rests upon evidence more direct. I have endeavoured to prove what before could only be suspected.

The demonstration of this view, resting upon the constituents of the lymph itself, the changes undergone in the ovum, and in the application of the laws of metamorphosis, give me not only a claim to originality in the general principle, granted by Dr. Carpenter, but also to priority in the adoption of direct inductive argument. The announcement, also, that uric acid or its elements is the azotising principle, necessarily flows out of my mode of reasoning, and is wholly mine. I shall not dilate upon the importance of this principle, both physiologically and pathologically.

I have much satisfaction in remarking that Dr. Carpenter, with a quick apprehension of its value, has considered that it adds much force to the doctrine first suggested by him that the blood was the means of effecting the changes in the lactiferous vessels; for after the withdrawal of uric acid from the blood to perform this office, its remaining elements pass through the liver to be secreted in the form of bile, and "Liebig has shown that bile + urine = animal flesh, or thereabouts."

This view, also, completes the chain of evidence I have brought forward respecting the pathology of typhus; for if uric acid be not separated from the blood in the lactiferous vessels, owing to any morbid condition of these vessels, the blood will not be prepared for the formation of bile; the liver will, in consequence, suffer an arrest, or derangement of function, and uric acid and oily matter will appear more abundantly in the urine. Let me further add the probability that the red particles are mainly instrumental in the performance of these azotising processes.

Dr. Carpenter will doubtless be pleased to see the further application of the point which his discrimination made evident. The process of the conversion of oily matter into albumen is one of those important changes of which (to use the words of Dr. Carpenter) "we are yet entirely ignorant." If my exposition has contributed to render

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strongly pulsating umbilical cord, which was then divided betwixt them, I observed the arteries on the placental side of the cord divided, to continue pulsating for several moments subsequently.

this process clear, I shall not have written in vain. I remain, Sir, your most obedient servant,

GEORGE ROSS.

Enfield, March 7, 1843.

THE SPLEEN AND THE PLACENTA.

To the Editor of THE LANCET.

SIR,—Allow me to furnish you with my answer to Dr. Way's remarks of last week, p. 823.

Dr. Way cites,—against my observation that in the higher animals there is no anastomosis between arteries and veins,—the circulatory arrangement of lizards and tortoises. Also (which has nothing to do with the question) the abnormally patent foramen ovale in cyanosis. He seems to have forgotten to introduce the other abnormal deviation, namely, varicose aneurism. But as well might a surgeon attempt to practise the adhesion of mucous and serous membranes, as expect the middle coat of an artery to merge directly into that of a vein, in the higher animals.

The *old* experiment of bleeding a parent animal to death, and finding the fœtus not exsanguineous, is quite compatible, also, with *my* statement. There is no reason why the fœtus should, at once, send all its blood through the umbilical arteries, because the parent, when bled to death, ceases to furnish blood to the umbilical vein. The fœtus is constantly receiving and constantly returning a *proportion* of its circulating fluid; because the former is cut off, that is no particular reason why the latter should overflow. To quote old authorities against my experiments is, *in a small way*, like accusing Copernicus of ignorance of the theories of Hipparchus and Ptolemy. The placenta is always gorged when thrown off in the natural way; and it seems that wax injections never permeate its capillaries under any circumstances. But I shall shortly have another opportunity of aiding this point by an experiment, of which, perhaps, none of Dr. Way's authorities ever dreamt. I remain, Sir, your's ever truly,

ROBERT STEVENS.

Kennington, March 6, 1843.

MEDICAL NEWS FROM THE EAST.

To the Editor of THE LANCET.

SIR,—If I may judge from the interest now taken by the public in everything which relates to the progress of civilization and the important changes continually going on in the East, the following communication will be received by your readers with very peculiar satisfaction. It is the first of the kind which has been transmitted from the Levant, and is an earnest, I hope, of "better things to come."

We have the testimony of the most experienced travellers that, for many generations Syria, and Palestine, like most other parts of the Ottoman Empire, have been left to the tender mercies of charm-venders, miracle-mongers, adventurers, fakirs, and sorcerers, always ready to impose upon the credulity of the simple; and even in modern times, it is a well-known fact that, until lately, there has not been a single resident practitioner to be found (possessing the slightest claim to respectability) from Gaza to Antioch, from Hebron to the Haouran, or from Beyrout to Damascus, Homs, and Hamah. But there is now an English physician at Jerusalem (Dr. M'Gowan, who resides with the Protestant bishop); and in the summer of 1842, Dr. Kerns, the first agent of the "Syrian Medical-Aid Association," also quitted the shores of England for "the land of the Patriarchs." This gentleman entered upon his philanthropic mission with humble confidence of success, though with some misgivings concerning the distracted state of the country and the deeply-rooted prejudices of the people, which, it was to be feared, might, at the outset, materially interrupt his labours. I am happy to say that he has succeeded beyond his most sanguine expectations, and this interesting and densely populated territory is now beginning to appreciate the efforts which are making for its relief. No sooner was it announced that an English physician had been sent to establish a dispensary at Beyrout, than numbers of the inhabitants flocked down to the coast, bringing with them the sick, the halt, and the blind, that he might "lay his hands on them and heal them."

Knowing, as I do, the deplorable condition of Syria and Palestine, having myself witnessed the eagerness with which professional aid is sought by all classes when once their confidence is established, and the unfeigned gratitude which the inhabitants evince for the most trifling alleviation of their sufferings, it is particularly gratifying to me to think that my own countrymen should be the first to associate in so good a cause, and that the pleasing office of declaring these good tidings has been entrusted to me by the committee.

Permit me, then, to lay before you the first report of our first medical agent in Syria, and when I tell you that Dr. Kerns has, by the blessing of Providence, himself relieved, in the course of three short months, under every disadvantage, 1297 of his suffering fellow-creatures, who were previously, in common with many thousands, without any resource to which they could apply, let the emergency be what it might, and that he has voluntarily contributed to their neces-

sities in various other ways, I am certain you will take pleasure in making known what has been done, and what is still doing for poor afflicted Syria.

I ought not, in conscience, to extend these remarks, otherwise I might say much, but it does appear, from facts which have come under our notice, that the philanthropic exertions now making at Jerusalem and Beyrout are pre-eminently calculated to raise the British name in the estimation of the Syrian people. Experience teaches that to heal the sick, to assuage pain, and to bind up the broken heart, under the benignant influence of pity, is the surest road to the affections of men, and there are few, I apprehend, so ruthless as to resist such an appeal to the better feelings of their nature. Having once tasted the sweets of sympathy—pure and disinterested friendship such as this—these hitherto deluded beings become devoted to their benefactors, and multitudes cry aloud for aid, which, alas! it is not in our power to give. They undertake long journeys to implore relief, and many faint and perish upon the road. In Damascus alone there is a population of 120,000 utterly destitute of medical advice.

We do hope to be able, ere long, to establish an agent among them, and as soon as our funds permit, it is our intention to build an hospital at Beyrout. We feel that, without such an asylum, our best exertions will only ensure a limited degree of success; for many patients are brought from the remote and lofty regions of Lebanon, and their symptoms being aggravated by fatigue, they linger about the courtyards, among the ruins, and in tents, in order to be near the "Hh'akkim," whose efforts in their behalf, however, are often counteracted by numerous circumstances which are obviously beyond his control, more particularly in cases of fever, and diseases of the eyes. It is considered charitable in these countries to crowd round the sick to cheer and amuse them, and where patients are thus huddled up together among heaps of filth and rubbish, there can be little attention to cleanliness, ventilation, or temperature; they can neither be defended from the sun by day, nor the dew by night. Moreover, a great many, finding themselves *relieved*, and anxious to get back to their families, retire to their distant homes too soon, and doubtless relapse in consequence. Thus it is hardly possible to arrive at accurate results either in regard to deaths or cures. Any record upon the subject must, at present, be liable to objections, but we hail the document which we have received as a harbinger of much good, and every credit is due to Dr. Kerns for having accomplished so much in so short a time. Another important feature in the plan of the Association is to establish a medical school, that the natives may themselves be instructed in the "healing art;" they display considerable abilities, which only want to be well directed. It may be proper to state that Dr. Kerns (who was elected out of forty eligible candidates) is thirty-eight years of age, married, and a gentleman of tried character and experience. I remain, Sir, your obedient servant,

WILLIAM HOLT YATES, M.D.

Woburn-place, Russell-square,
March 6th, 1843.

P.S. Since writing the above, intelligence has been received that Sir Moses Montefiore is about to establish a dispensary at Jerusalem for three years, at his own cost, by which time it is expected there will be a complete hospital in successful operation. The dispensary is to be managed by Dr. S. Frankel, a Prussian, and his services, like those of Dr. Kerns, are to be at the disposal of persons of every country and denomination; the doors will be open, without distinction, to all that suffer and apply for aid, whether Jew, Protestant, Roman Catholic, Greek, Maronite, Mussulmaun, or Druse.

Extract from a Letter sent by Dr. Kerns to the Committee of the "Syrian Medical-Aid Association," London.

"Gentlemen,—I send you herewith a report of the three months just ended; I could not, from several causes, commence my registry sooner. Since my last communication this unhappy land has gained nothing in tranquillity, and what the ultimate result may be it is still impossible to conjecture. However, my dispensary goes on smoothly and satisfactorily; of course, in my professional vocation I know no distinction of persons. At first, very few Turks or Mohammedans came to me, such was their bigotted hatred of the Christian name! But I must not omit to mention that, now, in applying for medical relief, religious prejudices do not appear to have much, if any, influence. My patients consist not only of every Christian sect in the country, but also of Mohammedans, Jews, and the Druses of Lebanon; and, as a pleasing evidence that the people appreciate the advantages which British benevolence has placed within their reach, I may notice the fact that, even under the seclusion which custom has imposed upon the females of these countries, and the reluctance with which they usually approach strangers, among the applicants at the dispensary, we have had a larger proportion of females than males!

"T. KERNS, M.D.

"Beyrout, Dec. 8, 1842."

REPORT *of the Number of* PATIENTS *relieved at the* DISPENSARY *of the* SYRIAN MEDICAL AID ASSOCIATION, *recently established by the British at* BEYROUT.

	Sept.	Oct.	Nov.	Total
Ophthalmia	67	130	156	353
Purulent ophthalmia of infants	..	..	..	9
Entropium	5	4	5	14
Ectropium	1	3	1	5
Pterigium	..	..	..	10
Amaurosis	..	..	..	4
Cataract	1	6	5	12
Intermittent fever	58	30	22	110
Continued fever	14	13	32	59
Puerperal fever	..	..	..	3
Rheumatism	27	28	46	91
Bronchitis and pneumonia	18	13	36	67
Pulmonary consump.	..	..	..	4
Inflammation of the throat	1	2	2	5
Dyspepsia	49	78	95	222
Disease of the liver	8	3	6	17
Dropsy	6	4	4	14
Dysentery	3	4	7	14
Diarrhœa	4	2	2	8
Urinary diseases	8	9	5	22
Eruptions	7	14	18	39
Worms	14	11	12	37
Paralysis	..	..	..	4
Spinal disease	..	..	..	2
Deafness	3	2	3	8
Ulcers	16	25	47	88
Abscess	2	6	8	16
Fungous tumours	..	..	..	3
Hernia	1	1	4	6
Club-foot	..	..	..	3
Cancer	..	..	..	6
Patients not included in the above	..	..	..	42
Total	..	..	..	1297

Males, 623
Females, 674

The mean temperature of Beyrout:—

	Morn.	Noon.	Even	Hottest Day.	Difference. Mrn. & Noon	Difference. Morn. & Even.
April	65	72	66	86	7	6
May	71	77	72	89	6	5
June	73	78	75	87	5	3
July	79	86	82	94	7	4
Aug.	83	89	84	97	6	5
Sept.	80	87	83	95	7	4
Oct.	79	84	81	91	5	3
Nov.	65	71	64	77	6	7

The mean temperature of Hamdoom, one of the higher villages of Mount Lebanon:—

July	72	75	73	78	3	2
Aug.	65	71	70	84	6	1
Sept.	69	73	72	83	4	1
Oct.	62	70	69	82	8	1

The record of April, May, and June, being previous to my arrival here, was furnished by a resident gentleman. That of Hamdoom was kept by an American missionary.

THOS. KERNS, M.D.

Dr. KERNS' *Remarks on the above Report.*

Ophthalmia.—Under this head is included every stage of the disease—inflammation of the conjunctiva, ulceration of the cornea, protrusion of the iris, specks and staphyloma. Inflammation of the eye here, if improperly treated or neglected, sometimes runs its destructive course with great rapidity. I have seen protrusion of the iris in about five days from the attack, and partial opacity of the cornea is remarkably common. Active depletion, with mercurial purgatives, and the application of a solution of nitrate of silver, I find the only effectual remedies. It is difficult to assign the chief cause of ophthalmia in this country; it attacks all classes and all ages. It has been ascribed to sleeping out in the open air, which the poorer classes are very fond of, during the summer nights, but many cases have also come under my notice among those who never do so. Again, want of cleanliness has been thought a fruitful source of the disease, but numerous instances occur in individuals who are extremely clean and neat in their persons; neither can poverty nor privation be, with propriety, assigned as the cause. It is incorrect to draw such a conclusion because a greater amount of disease is to be found among the poor; that class being the most numerous in every country, will always render this a natural result. If I were to name any one cause as being more deeply chargeable with this disease than another, I would say that it is the direct glare of the sun to which the eyes are subjected by the ordinary dress of this, in common with other oriental countries, as it affords no shade to the eyes, such as the turban, tabousch, and Persian cap. The veil worn by the females, though covering the

eyes, is scarcely less distressing, being closely applied to the face, and cannot render that soft shade which European hats and bonnets afford.*

Pterigium.—This disease of the eye is very common.

Cataract.—I have operated on eight persons with various degrees of success, but such operations are performed under great disadvantages without an hospital.

Intermittent Fever.—It is difficult to assign an evident cause for the prevalence of this complaint. In the neighbourhood of Beyrout, at least, where the land is parched with drought one half of the year, and where, at no period, marshes or stagnant pools are to be found to any extent, marsh miasma would seem to be out of the question; however, if we omit the association of *marsh*, which is but a casual circumstance, the theory of decomposed vegetable and animal matter will remain as probable as any other, and as applicable. These effluvia are often more offensive from the absence of water.† Intermittent fever is here found among all classes and ages, and frequently recurs several times when the patient is apparently quite well, and it hangs about him for eight or ten weeks together.

Continued Fever is generally of the common inflammatory form. I have only met with two or three cases which assumed a typhoid character.

Rheumatism.—The prevalence of rheumatism manifestly arises from sleeping on the ground in the open air. I seldom find it in those who have not thus exposed themselves.

Pulmonary Consumption.—So far as I have been able to judge, this is a rare disease. Bronchitis is the most frequent pulmonary complaint in Syria.

Urinary Diseases.—These are chiefly chronic affections of the mucous membrane, and irritation arising from the habitual use of stimulants.

Ulcers.—These arise from trivial causes, and are generally most obstinate to heal.

Fungous Tumours.—One of these occupied the entire place of the cornea, hung over the cheek, and presented a bleeding surface. I passed a hook through it, and removed it, together with a portion of the globe. It appeared to have originated in a thickened conjunctiva.

Club-Foot.—I operated in two cases with success, which gave great pleasure to the country-people who saw them.

N.B.—Those diseases which might be influenced by season I have marked in months, as they occurred, so that at the end of the year, some useful calculations may be made on the subject.

THOS. KERNS.

NOTE.—*To the Editor.*—Sir: There is nothing extraordinary in coincidences in discoveries, which, however, lose somewhat the semblance of originality when improvements or suggestions having been recorded in them, they are, after the lapse of years, again announced as new. I am led to these remarks by observing two propositions, originally made by me, which have been treated in a similar manner. The first was my project for the employment of internal pressure on the abdominal aorta in uterine hæmorrhage, commented on in THE LANCET for Feb. 23, 1828 (No. 234), and published in the "London Medical Repository" three years previously (1825), with cases, and subsequently followed by a notice in Oct., 1828, of this novel practice by Dr. Eichelberger and Von Siebold, at Berlin. The second is the description of a "New Lithotomy Staff" by Mr. Bryan, of London, with remarks on the operation, published in THE LANCET, Feb. 11, 1843, which, in many respects, resembles one proposed by me in the same work, for July 12, 1834, especially in cutting on the *concavity* of the staff, as also in the parallelism and *angle* of the incisions. This proposition, with the observations thereon, have been honoured with the approval of some eminent operators; but at present I content myself with merely noticing the facts, and referring the profession to the communications abovementioned. I am, Sir, yours very obediently,

G. W. JAMES, M.R.C.S.

March 6, 1843.

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* There is much truth in these observations; but I think we may justly ascribe this "scourge of oriental climates" to the sultry heat of the vallies, exposure to the mid-day sun, check of perspiration caused by the heavy dew of the night, and other circumstances, which, producing irregular determination of blood, favour congestion of the liver and spleen; and this seems to be borne out by the fact that persons of a bilious temperament, who have swarthy complexions, black eyes, and black hair, and generally relaxed bowels, suffer less from ophthalmia, but are the frequent subjects of dysentery and diarrhœa; whereas those of a sanguine temperament, who have light eyes and light hair, a *deficiency* of bile, and a clearer skin, often escape the latter, but are very prone to constipation and ophthalmia.—*Wm. Holt Yates, M.D.*

† Having been myself a sufferer at Beyrout from this complaint, and witnessed its effects in others, I took some pains to investigate the subject. With your permission, therefore, I will avail myself of an early opportunity of sending you a few remarks on the climate of Syria, as connected with the points referred to in this communication.—*Wm. Holt Yates.*

THE LANCET.

London, Saturday, March 11, 1843.

We have received so large a number of communications on the subject of the trial of M'Naughten, embracing every conceivable view of the policy and the impolicy of the extent to which the plea of insanity is urged and admitted on trials for murder, that we are anxious to obtain time for examining those communications with the utmost care, and we consider it, therefore, prudent to delay the publication of our opinions on the question until next week. The trial of M'Naughten has terminated, there is no other case of the kind pending, and the delay of a few days will not be productive of any damage to such an important and deeply interesting subject.

A correspondent, who states that he is intimately acquainted with the circumstances which he describes, has addressed to us the following remarks, earnestly entreating that they may be published in this journal, as an act of public justice towards Mr. Phelan, who, our correspondent alleges, is the victim of an unprincipled persecution. Without passing any opinion on the subject at present, we record in this place the views of the writer, for the sake of fair play to the complaining party:—

(From a Correspondent.)

Unfortunately for all their best interests, public as well as private, the people of Ireland, cherishing feelings and prejudices which have long since been banished from among their fellow islanders, conduct their various political differences, and even their professional divisions, with such acrimony as to lose all sense of nationality, and forfeit every reasonable expectation of benefiting particular classes of the community in adjusting them. In Ireland, the exertions of individuals to effect a public good are generally thwarted by an unlucky disposition to deny the possession of merit by any individuals in the community but those who belong to a particular party, that party assuming to itself the privilege of always passing judgment in favour of its own associates, or condemning and sending to the devil every man not of the same faction. A particular instance of this habit occurs at this moment in a professional question that has recently been debated in one of the Dublin medical journals, which, since it long since forgot, in its criticisms, to be truthful and honest when talking of The Lancet, we shall be excused for not designating by name until it has recovered the lost qualities.

A few words of history on the matter must be given in discussing the points to which we refer.

A short time since, leave was obtained by Mr. F. French, member for Roscommon, to introduce into the House of Commons a Bill for "Better regulating the Medical Charities of Ireland." In his views on the subject he quoted largely from the reports of the Poor-law Commissioners on those charities, the reports being the results of an inquiry that was made under the direction of Parliament by Mr. Phelan, one of the assistant-commissioners (who is also a member of the London College of Surgeons), and Dr. Corr, a member of the Dublin College. These reports show, as did those of the four medical gentlemen who were employed for a similar purpose by the Irish Poor-inquiry Commissioners, that serious defects exist in the constitution of the seven or eight charities concerned, and that no small amount of abuses have, on both occasions, been found in their management, consequences which become inevitable where no controlling authority, and no regular examination and auditing of the accounts and expenditure of funds, have been provided.

The medical party, however, to which the journalists in question belong, namely, the Dublin College of Surgeons, and the county infirmary surgeons, assert, with reference to these reports, as they did with respect to those of the four gentlemen alluded to, that they are not entitled to credit, "the state-"ments being grossly exaggerated and un-"fairly coloured." Yet Mr. French, who, as an Irish member, had good opportunities of making himself acquainted with the facts, — Lord Eliot, the Secretary for Ireland, to whom various parties have been

supplying information on the subject,—and all the Irish members, by their silence,—concur in admitting the general accuracy of the reports.

The medical men employed by the Poor-inquiry Commissioners were Doctors Sir DAVID BARRY, BORREL, and CORRIE, and Mr. C. ROONEY. The three former gentlemen were British graduates in medicine, and entirely unconnected with Ireland. Dr. BORREL and Dr. CORRIE are Englishmen. Sir D. BARRY and Mr. ROONEY are Irishmen, the latter a member of the Dublin College of Surgeons. What possible motives could the first-named three gentlemen have had to furnish incorrect reports, and "to pick "out a few cases of abuse or delinquency, "and then to charge them against the whole 'system" as is asserted by the Dublin journalist, who excludes Mr. C. ROONEY from the censure conveyed by this assertion? The charge is manifestly unjust. As even the reports of Mr. ROONEY prove the existence of the very defects and abuses which are stated by the others to exist, the writer can only be supposed to have been actuated by a desire to discredit anything that came from the pens of British graduates, and to have it believed that those of the Dublin College, and the institutions with which they are connected, give birth to men who are superior to all others in the first qualities of professional men. But nothing beyond mere assertion on the matter has been ever adduced, or attempted to be adduced, in discredit of the impugned reports and their authors, and those assertions only manifest and exemplify an ill-feeling on the part of the agents of the Dublin College of Surgeons towards the British graduates.

In commenting on the Poor-law Commissioners' reports, the journalist says that so and so "no doubt appeared very deplorable "to *an apothecary*, for the author of the *report* "is *nothing more or less*." The nose, we presume, is here turned up at Mr. PHELAN, and those who, like Mr. PHELAN, have taken British degrees, and who are thus impudently represented by the Dublin-College people as neither more nor less than "apothecaries," because they happen to add the latter qualification to the former. The friends of Mr. PHELAN, we understand, would feel mortified at this attempt to disparage that gentleman, if it were honestly made, and did not arise from that species of viciousness which characterises all party critics. For they consider that no member of the Irish College has made greater exertions to benefit his fellow-creatures, and the members of his profession. A few years ago THE LANCET noticed his exposure of the defects and abuses which exist in the numerous medical charities of that country, and gave him that credit for his labours to which he appeared to be fairly entitled by them. Other writers, we observed did the same, and as incessant efforts are being made by a section of his own countrymen to damage him in public estimation, we shall quote a few passages from two of the works, in evidence of the estimate then formed of the ability and praiseworthy exertions of that gentleman. The "British and Foreign Medical Review" (1836), wrote thus:—

"To show the wants of the sick-poor in Ireland, the extent, the abuses, the inefficiency of her medical charities, and the means of remedy, the statistical inquiry before us is a valuable document. It develops, on the one hand, the frequent sickness and wretchedness of an impoverished people; on the other, the institutions intended for their amelioration, often wasting and misusing the stock of good at their disposal. * * * The elaborate tables at the end of the volume have been compiled with great care, and the materials from which they were deduced must have been collected at great expense. * * * On the whole, the statistical department of Mr. Phelan's work is executed with great ability, and makes us hope that the Government will avail themselves of the assistance of that gentleman in any system they may propose for the regulation of the Irish medical charities."

In the 4th vol. of the "Transactions of the Provincial Medical and Surgical Association," published in 1836, Dr. WALKER, of Huddersfield, presented an elaborate article entitled "Statistical Observations on the Medical Charities of England and Ireland;" referring to the latter, and to Mr. PHELAN'S work, DR. WALKER observed therein:—

"It has been reserved to a most talented individual to draw attention to the subject, and to supply what was long wanted, a statistical inquiry into the present state of the medical charities of Ireland; and the manly and masterly manner in which he has denounced the various abuses in the Irish system of medical relief, is such as must force itself on the attention of the Legislature. The facts which he adduces are supported by tabular evidence, and the manifold glaring defects are now fairly submitted to the tribunal of the public. If they can be disproved, now is the opportunity to do so."

We believe that no attempt to disprove them has been made. On the contrary, Dr. Corr, a member of the Dublin College of Surgeons, has, after a laborious and careful inquiry, throughout Ireland, fully confirmed the statements contained in Mr. Phelan's work; so also did the four medical gentlemen before mentioned, whose reports were issued subsequently to the publication of that work. Dr. Walker further says, that on the accuracy of Mr. Phelan he considers that "the most implicit reliance may be placed, "—that Mr. Phelan has enriched his "work by some most valuable tables, the "result of a long and a toilsome calculation, "to which few but himself would have sub-"mitted," &c. These extracts show that although Mr. Phelan is not a member of the Dublin College of Surgeons, his labours have been highly estimated in quarters where no private prejudices or feelings of self-interest existed to sway the judgment, and we quote them because the writers appear to have pronounced their opinions after a minute and adequate examination of the contents of the work under review.

Some most invidious efforts have lately been made to induce the authorities to remove Mr. Phelan from the Poor-law Commission, he being the only medical man connected with that body. This act of professional suicide is characteristic of "parties" in politics and religion, and is without credit either to the hearts or the brains of the culprits committing it. Whilst the whole medical profession—some few scape-graces, save and except—and all right-thinking persons are calling upon the Government to adopt a system of medical supervision in connection with the administration of the Poor-laws, the Commissioners are, in an odious, obstinate, factious spirit, dismissing the only medical officer in their body,—and that in a country where, from the number of "medical charities" that it contains, the services of a Medical Commissioner are specially important to the state. This is a reckless, offensive, disgusting proceeding, which in England, as well as in Ireland, is only calculated to degrade the authorities in the eyes of a profession that is specially looking for the appointment of one or more surgeons in connection with the Central Board of our own Poor-law Commission. Those cunning, plotting, hungry gentlemen—the barristers—put up with no such treatment of their corps. While the one surgeon in Ireland is to be reduced, ten non-medical *employés* hold their ground. Yet between a good physician and a sharp lawyer, who would for a moment hesitate in making choice of the former for superintending the relief of the poor,—with every man, woman, and child amongst them sick, by turns.*

INSUBORDINATION AT THE BENGAL MEDICAL BOARD.

(*From a Calcutta Newspaper.*)

"There is a somewhat novel state of things, regarding the members of the Medical Board, at present under the consideration of the higher authorities, and which, immediately relating to the question of military uniform, involves the higher one of military authority. We shall relate one of the several accounts (not substantially varying) which we believe to be the most correct.

"Not very long since, Mr. Sawers, the senior member, considered of a sudden that as there was a uniform for the medical staff that uniform should be worn at all the meetings of the Board, and he mentioned this desire to the other, or junior, members, Doctors Campbell and Smith, and said that at the end of a fortnight (allowing that time for the uniforms to be prepared) they should appear accordingly. They, considering this as a proposition rather than as an order, voted against it, and intimated to Mr. Sawers that his motion was negatived by the majority.

"He made no remark whatever upon this result, and such meetings as next ensued were attended in the old way—plain cloth coat, or white jacket, according to the 'warm feelings' of the respective members—until the first meeting occurred after the expiration of the fortnight's law, when, on Dr. Campbell's entering the office in a white jacket, Mr. Sawers, who was himself in undress uniform, ordered him to go home and consider himself in arrest for disobedience of orders. Home he went, accordingly,

* A morsel of medical news has reached us from Dublin, at the close of these remarks, which we may as well append in this place, for the information of those who interest themselves in such affairs. It is this, that the Irish Medical Association have recently met to consider a letter which had been addressed to Lord Eliot by a certain disappointed little animal, occupying the malposition of combatant of his lordship. Over this letter, at the meeting, Mr. Carmichael and the author were not near enough to come to blows, so blows were saved; but the society wants to get rid of a secretary that is pugnacious on such occasions.

and there he has remained in arrest ever since, and charges have been sent against him by Mr. Sawers, grounded on his recusancy. These charges are before Government and the Commander-in-Chief, and we understand it is not found an easy matter to decide how they should be dealt with."

Thus, whilst Mr. MARTIN was struggling at home for his brethren in India, and that amidst difficulties which those alone can appreciate who have had to carry points with persons in authority, or through the great public offices of this metropolis, the old men, his official superiors in Bengal—that mass of inert obstinacy, the "Board"—the three dull-brained heads of the medical department of the army,—were busying themselves in the miserable manner above described.

Pharmacologia; being an extended Inquiry into the Operations of Medicinal Bodies, upon which are founded the Theory and Art of Prescribing. By J. A. PARIS, M.D., &c. New Edition. London: S. Highley. 8vo. Pp. 622. 1843.

THIS is the ninth edition of one of the best-known books in the profession, and, like a pen, the older it gets the more it is mended; but, fortunately, unlike a pen, the oftener it has been mended the better it has got,—by the grace of *discovery*, which has specially tended lately to make surer and more sure the principles upon which prescriptions for disease should be framed,—very desirable principles to be learned, and for a methodical explanation of which—with a peculiar bias to the "elegant" in medicinal compounds—the members of the profession are much indebted to Dr. Paris, as is Dr. Paris to the profession, for the welcome annuity which they have paid him in return for the work.

It would be a waste of time to point out what has been newly selected, and added to the old materials of this volume. The author says, in fact, that it has been "rewritten, in order to incorporate the latest discoveries in physiology, chemistry, and materia medica." Its fundamental doctrines, he says, "have not been shaken, but so rapid and extraordinary has been the advancement of every branch of science during the last ten years, that its very language had become obsolete." Amidst the later discoveries in chemistry "those of Liebig necessarily attract notice," several of which, however, says Dr. Paris, bear directly against opinions which he (Dr. P.) has confidently maintained, and which are "more especially in opposition to those relating to the process of digestion. Liebig," he continues, "captivated with his theory of transformation, enlists it at once into his service, to explain the phenomena of that process. He (Liebig) denies the existence of any distinctive digestive principles, and maintains that the change which the food undergoes in the stomach is the result of induction, from the stomach itself yielding a substance in the state of transformation, with which it comes in contact. All experiment is opposed to this view. Schwann actually proved that the principle of the gastric solvent could be precipitated by acetate of lead from its neutral solution, and again recovered from the precipitate, with all its former activity, by hydro-sulphuric acid."

We venture to say only, on this question, that of the two teachers, John Arthur Paris and Justus Liebig, the former will quit his opposition to the "mechanical" doctrine of digestion long before those of Liebig are abandoned by their author, or anybody else who has once adopted them, and then every one will be glad to see the tenth edition embracing the new heresy.

THE PERIODIC LAW OF SEVEN.

SIR,—I forward you an illustration of Dr. Laycock's "Law of Seven" (LANCET, pp. 124, 160, 423). I lately attended a lady in her confinement, and ascertained that from the date of the last menstruation to the day on which labour commenced, was exactly two hundred and eighty days, or forty complete weeks. On the last day that she menstruated her husband returned from a journey. The labour was long and severe, and inflammatory action commenced after delivery. On the fourteenth day from the commencement of labour the symptoms were so much aggravated as to lead to an unfavourable prognosis, which was fulfilled in five complete weeks from the commencement of labour. My patient appeared in *articulo mortis* on the fourth day before her death, but rallied surprisingly until the thirty-fifth day from labour, when she again sank, and it became certain that she was dying. As the case might be recognised if I published my name, to the annoyance of my patient's friends, I give it you confidentially, and sign myself

CHIRURGUS.

Feb. 26, 1843.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, February 28th, 1843.

Dr. WILLIAMS, President.

On Fatty Degeneration of the Arteries, with a Note on some other Fatty Degenerations. By GEORGE GULLIVER, F.R.S. (Communicated by Dr. HODGKIN.)

THE author, remarking how vaguely the epithets atheromatous, steatomatous, &c., have been applied by pathological writers to diseased arteries, and that the morbid deposits between the middle and inner coats, and in the substance of the former, has not, as far as he knows, yet been submitted to precise examination, gives the result of his own observations, from which it appears that the disease is really of a fatty nature. A microscopic examination of it brings into view a multitude of crystalline plates, fatty globules, with albuminous and earthy particles. Several specimens of the crystals were sent for examination to Dr. Davy, who ascertained that they are of cholesterine.

The fatty matter is easily extracted by boiling alcohol, and the crystals of cholesterine are seen to be deposited as the solution cools. The author has examined numerous specimens of the disease, and has never failed to observe these crystals and the fatty globules in the deposit, and also generally in the substance of the altered middle coat. The microscopical characters are given in two figures.

The accuracy of Dr. Davy's observations (see his Researches, Phys. and Anat., vol. i., p. 372 and 436), as to the thinning, &c., of the middle coat of the artery, is confirmed by Mr. Gulliver.

The importance of fatty degeneration of the coats of the arteries is insisted on, especially as to its general connection with thickening and puckering of the inner membrane, with aneurism, with obstruction, occlusion, or ossification of the vessels, and with those ruptures of them which are so frequently the cause of sudden death.

The author adds, that fatty degenerations are more common and of more importance than has yet been supposed. He mentions obstruction, by fatty particles, of the seminal tubes; and notices fatty degeneration of the blood, lungs, &c. The disease he describes as being more remarkable in "brown consolidation" of the lungs than in red consolidation, and these two diseases are described as affording distinct morbid products.

The PRESIDENT drew attention to the interesting pathological fact brought before the society in the paper just read. It appeared from this paper that that disease of the arteries usually denominated atheroma, was very improperly named. He had always looked upon this affection as a species of softening of a diseased portion of the lining membrane of the vessels; it would appear, however, from the experiments and observations of Mr. Gulliver, that the deposit in question was to be classed among the fatty substances.

Mr. BRANSBY COOPER was sorry not to see Mr. Gulliver present, as he had been anxious to ask him a question; perhaps Mr. Dalrymple might be authorised to give an answer. Did the author of the paper mean to imply that the fatty deposit which he had spoken of, and that deposit which was usually denominated atheroma, were the same thing? He (Mr. Cooper) believed that there was a real distinction between the two kinds of deposit, the fatty, he thought, being the mere result of age, and having its situation between the middle and inner coats, while the athermatous deposit was not confined to old age, and had its situation entirely in the inner coat.

Mr. DALRYMPLE had no authority to answer the question of the last speaker. He might remark, however, that he had been a witness of some of the observations of Mr. Gulliver, and he need scarcely say that that gentleman had given a most correct account of them. Ordinary chemistry, without the aid of the microscope, would have been insufficient to have determined the results which had been arrived at, for the portions of oleine, margarine, &c., were too small to be observed by the naked eye, but the crystals of these substances were rendered unequivocally distinct in the field of the microscope. He would suggest that this was not the proper period to discuss the paper of Mr. Gulliver, as it was only one of a series which Mr. G. intended to bring before the society on fatty degeneration. He (Mr. D.) might remark, that the fatty degeneration which had been examined by the author of the paper was not the mere natural result of advanced life; for, as had been stated in the paper, it had been found in the arteries of persons of forty years of age, and had been shown to be the cause of the rupture of vessels and of apoplexy. It was, however, not without interest to notice that in extreme old age the bones themselves, while they relatively consisted of more earth and less gelatine, had absolutely more of oily material than in the earlier periods of life.

A Normal and Abnormal Consciousness, alternating in the same Individual. By JOHN WILSON, M.D., Physician to the Middlesex Hospital.

This case occurred in a boy aged 14, a patient in the Middlesex Hospital, who was said to have complained of headach for two or three days, but whose appearance was healthy. For three or four days his appetite was inordinate, seizing upon any article of food he could meet with in the ward, though allowed full diet. When not eating, or

seeking for food, he generally slept night and day.

This abnormal state continued for three or four days, when he recovered his natural state of sleep, appetite, and consciousness. Then he had no recollection of what he had done, or of what had happened to him since his admission. He was shortly discharged, but twice readmitted, each time presenting the same symptoms, viz., alternations of consciousness and unconsciousness. No treatment was adopted. The author, for the present, reserves his opinion and inferences drawn from this case; his object is to invite further examination for similar cases, and when such occur then will be the time for discussion.

Dr. Webster regretted that the author had said so little regarding the mental condition of the patient whose case had just been brought before the society, as it was difficult to decide, from the meagre description of symptoms, whether the boy laboured under a disease of the body or one of the mind. He (Dr. W.) could not certainly consider the instance now related as one of double consciousness, which seemed to be inferred; indeed, it was almost impossible from the narrative to form any definite opinion. In reference, however, to the subject of double consciousness, he would mention to the society an example of that rare form of mental disease which had been recently communicated to him by Dr. Browne, the able resident physician of the Crichton Asylum, at Dumfries, and now under his care. The patient alluded to believes he is himself and another person at the same time. No. 1 disavows the opinions and proceedings of No. 2, and *vice versâ*; No. 2 inflicts punishment upon the body of No. 1 for imaginary offences, and designates his opponent by bad names and abuse. Altogether, the case formed an interesting psychological curiosity. Although the author had said nothing of the treatment he (Dr. W.) thought benefit would have followed the use of remedies, both external and internal; and as the patient had been three different times in a public hospital, something ought to have been done for the cure of his disease.

Mr. Solly said he had been informed by Dr. Davey, of Hanwell Asylum, that in those cases of bulimia in which the patient's delusion consisted in his belief that he had "a gnawing devil" inside of him, the treatment of the disease as one affecting the alimentary canal, had frequently relieved and cured the patient. He (Mr. Solly) thought that in these cases we should be justified in resorting to a similar kind of treatment.

Mr. B. Cooper remarked, that in Dr. Wilson's case the patient was on low diet; he liked full diet better, and so robbed to obtain it. A good thrashing would have cured him.—(Laughter.)

Dr. Mayo had heard of a case of double consciousness which lasted for several months, and seemed to have had its origin in the woman having swallowed some ointment containing a quantity of gunpowder. In this case there was no bulimia, but obstinate constipation. In one state of consciousness this patient was capable of learning and saying things which she was quite incapable of doing in her other state. In one condition she was pert and impudent, in the other modest and retiring. The disease lasted some months, but was ultimately cured by attention to the stomach and alimentary canal. He believed that in some cases of a similar kind to that of Dr. Wilson, Dr. Abercrombie had adopted no plan of treatment, and he (Dr. Mayo) thought that in such a new state of things Dr. Wilson had been justified in merely looking to observe the course the case might take.

Remarks on the Calculi in St. George's Hospital. By Dr. Bence Jones. (Communicated by Mr. Cæsar Hawkins.)

The number of specimens submitted to examination was two hundred and thirty-three. The author's object, from the analysis of these calculi, is to arrive at conclusions with respect to the comparative frequency of different states of the urine in calculous complaints, and thus to obtain practical hints as to the efficacy of remedies intended to alter the secretion, or act upon the stone in the bladder. He presents several tables, and, taking four hundred and fifty states of the urine, inferred from the composition of the calculi, finds that in one hundred and thirty-nine it was alkaline and in three hundred and eleven acid, to test-paper. Omitting from the latter list fifty-nine specimens of oxalate of lime, two hundred and fifty-two cases of the uric acid diathesis remain; and in one hundred and seventeen of these no free acid was passed, from which the author concludes that alkalies would have been of no benefit in them, so far as neutralising the acidity of the urine was concerned. Taking the cases in which the alkaline concretions prevailed, he infers that in fifty-two the calculus might have been lessened by the injection of dilute acids, and in twelve the whole calculus might have been removed; while in others to which he refers, disintegration might have been effected. He concludes by describing a calculus in Mr. Cæsar Hawkins' possession, the nucleus of which consists of cystine, and which, from the history, appears to have been formed when the patient was two-and-a-half years of age.

Case of Ulceration of the Internal Jugular Vein, communicating with an Abscess. By William Bloxam, Esq., Surgeon to Queen Adelaide's Lying-in Hospital, and Lecturer on Midwifery. (Communicated by Samuel Lane, Esq.)

The patient, five years of age, after scarlet

fever, had suppuration of the glands of the neck, of the right side, near the angle of the jaw. Five days after the abscess burst, blood of a venous character was discharged from the opening, at first in small quantity, and afterwards more copiously. Graduated compresses were applied, but the hæmorrhage could not be restrained, and the child died on the fifth day from the commencement of the bleeding. On dissection, an ulceration of an oblong shape, about five lines in its long axis, was found in the inner side of the internal jugular vein, and opening immediately into the sac of the abscess. Extravasated blood was also observed beneath the integuments of the throat, or fore-part of the neck.

Mr. B. Phillips had never seen a case like that detailed in the paper, but curiously enough one had been recorded in Dr. Cormack's journal of that day. The case had occurred to a practitioner at Glasgow, and the record of it in the journal was accompanied by a drawing. In this instance an abscess had formed in the neck, and was opened in the usual manner; three days afterwards hæmorrhage came on, and the patient died; an opening, about half an inch in length, as if cut with a knife, was found in the jugular vein.

Mr. Snow related the case of a little boy who had an abscess just under the angle of the jaw, as he was recovering from scarlet fever. Fluctuation was very distinct in the swelling, and the matter seemed just beneath the skin; but as it remained in this state several days without breaking, it was deemed advisable to open it; on an incision being made, however, venous blood flowed in a large stream, without any mixture of pus. The bleeding was immediately stopped by a compress and did not again return. Two days afterwards, the abscess opened spontaneously, at a little distance from where it had been lanced, and discharged a quantity of matter without any blood. A similar swelling formed on the other side of the neck, which was allowed to discharge spontaneously, and the boy recovered. He believed the jugular vein had been opened in this case, although it was impossible to perceive the vein through the skin.

The President said, he thought that the absorption of poisonous matter from the tonsils was the cause of the enlargement of the lymphatic glands in the neck after scarlet fever; he would, therefore, inquire whether the tonsils were much affected in this case, and at what time the external swelling took place.

Mr. Snow said, that the tonsils had been so much enlarged as to threaten suffocation, and the swelling of the neck began between two and three weeks from the commencement of the fever.

Dr. Mayo mentioned a case to show that occasionally much mischief followed inflammation of the throat and swelling of the glands of the neck. A patient in the Marylebone Infirmary suffered from scarlatina, accompanied by severe tonsilitis. He recommended that the tonsils should be freely scarified, but to this the patient would not consent. The swelling and inflammation of the tonsils somewhat subsided, but the glands of the neck became very much enlarged. For this no treatment was adopted, and deafness ensued. He did not know how the case terminated, but it was an instance exemplifying the bad consequences which occasionally followed inflammation of the throat and swelling of the glands of the neck.

Mr. T. H. Cooper, to show the extent to which sloughing sometimes followed formations of matter in the neck, related a case of scarlet fever accompanied by severe sore throat. The inflammation of the throat subsided, but was followed by swellings of the glands in the neck. One of these under the ear was opened. The opening was followed by such an extent of sloughing that part of the parotid gland sloughed away, and the artery could be distinctly seen beating underneath.

Mr. Davis had generally found in scarlet fever that when the efflorescence was great there was much less likelihood of the swelling in the glands being severe, and *vice versâ*. When there was swelling of the glands the patient was more liable to anasarca, and when the sore throat was malignant the swellings and other sequences were generally formidable.

Mr. B. Phillips could inform Dr. Mayo of the result of the case which he had mentioned to the society. When the deafness took place the patient was transferred to the surgical wards of the infirmary. It was ascertained that the deafness resulted from enlarged tonsils blocking up the Eustachian tubes. A few brisk purges of calomel completely restored the sense of hearing.

Anniversary Meeting, March 1.

The meeting for the election of officers and the transaction of other business was held to-day. The secretary read the report, which gave evidence of the society being in a flourishing condition.

Mr. Macilwain threw out a hint that it would, in future elections, be much more satisfactory if the members had a list presented to them by the council, containing two names instead of *one*, for the filling up of every vacant office. He knew that there was a general feeling abroad on this matter, and, therefore, directed the attention of the council to it. He then proceeded to make some comments on the last volume of the society's "Transactions," which, he was confident, no member of the society could honestly say was such as ought to issue from that body, and which he was sure did not keep up the

reputation of the former volumes. He was proceeding to suggest the formation of a committee for some purpose connected with the society's "Transactions," when he was stopped by the secretary informing him that he could not discuss any matter of which notice had not been given. Here the subject dropped.

Dr. GREGORY referred to an item in the report, from which it appeared that the society were considerable losers by the publication of their "Transactions." He wished to have some information on this point, but after a little desultory conversation the matter dropped, without any information being obtained. It was observed that two hundred and sixty persons were entitled to receive the "Transactions," it was not, therefore, likely that the sale could be large.

The PRESIDENT gave a retrospective address, in which he first alluded to the flourishing condition of the society. During the past year one hundred and eighty-five volumes had been added to the library; the income had exceeded the expenditure by 150*l*., and thirty-three new fellows had been elected One fellow had neglected to pay his subscription, and two had resigned—Dr. Elliotson and Mr. Symes. The President then proceeded to read a very long account of the career of Baron Larrey, and gave some particulars respecting the late Mr. Walker, of St. George's Hospital.

On retiring from the chair, Dr. WILLIAMS gave a brief address, and the meeting adjourned.

The following members were elected office-bearers:—

President.—Edward Stanley, Esq., F.R.S.

Vice-Presidents.—Marshall Hall, M.D., F.R.S.; Edward J. Seymour, M.D., F.R.S.; Bransby B. Cooper, Esq., F.R.S.; R. D. Grainger, Esq.

Treasurers.—Samuel Merriman, M.D., F.L.S.; Cæsar H. Hawkins, Esq.

Secretaries.—George Cursham, M.D.; Alexander Shaw, Esq.

Librarians.—R. B. Todd, M.D., F.R.S.; Benjamin Phillips, Esq., F.R.S.

Members of Council.—H. Burton, M.D.; Sir James Clark, Bart., M.D., F.R.S.; Thomas Hodgkin, M.D.; John Webster, M.D.; Robert Williams, M.D.; Thomas Davis, Esq.; John W. Fisher, Esq.; William Lawrence, Esq., F.R.S.; E. A. Lloyd, Esq.; John G. Perry, Esq.

Trustees of the Society.—Edward Stanley, Esq., F.R.S.; James M. Arnott, Esq.; John Clendinning, M.D., F.R.S.

WESTMINSTER MEDICAL SOCIETY.

Feb. 18th. Dr. REID, President.

PARALYSIS.

Dr. CHOWNE adverted to a case of paralysis that he said was under his care. The patient, a woman about fifty years of age, a stout heavy person, who had enjoyed excellent health until the present attack, and had been a very high liver, taking twice or three times a-day animal food of the most nutritious kind, and wine and porter rather freely. She was thrown out of a chaise about five weeks since, but did not appear to have received any hurt. She had after the fall occasional slight uneasy sensations in the head, but not to a degree to excite particular attention. About a fortnight after the fall, and in the course of the night, while in bed, she awoke, and found herself paralysed on the left side, from the head to the foot; her power of articulation entirely lost; her face drawn to one side; the arm and leg quite powerless. Dr. C. saw her about a week after the attack. The case presented a very well-defined example of hemiplegia, in which the power of motion was lost, but that of sensation not affected; in every part of either limb the slightest touch was immediately perceived. Although the case could not be spoken of as presenting any very uncommon features, yet there were points which might form interesting subjects of discussion, both as regarded the pathology and the diagnosis. The attack had been sudden, the whole of one side had been involved, and, apparently, the head, the arm, and the leg in a nearly equal degree. In the course of the first few days the effects of the attack upon the head rapidly disappeared, the face recovered its right position, the articulation, from being actually impracticable, became possible, though difficult, and at the end of a week the head did not show any appearances, either by the eyes or otherwise, of having been included in the mischief. When the patient was first seen by Dr. C., not only did the face and speech appear to be quite natural, but the tongue was protruded in a manner not to betray what had so lately been its paralysed state. The leg had also very much improved, and the patient, with the help of a stick, could manage to walk a little. The condition of the arm was stationary, quite powerless, rather swollen, but very sensible to the touch. Up to the present time the leg had continued improving, but the arm had remained as it had been from the first. Dr. C. remarked that there was a difficulty in this instance, as in a large class of paralytic cases, where the symptoms were complicated, and did not corroborate each other in determining the exact seat of the mischief upon which the paralysis depended, and what was its precise nature. In a case such as that described a question might arise, first, as to whether the seat of the mischief were within the cranium or in the spinal column; and next as to whether it were the consequence of extravasation of blood, or of effusion, or whether of the character called nervous. From the head as well as the limbs having been affected, it appeared probable that the seat of mischief was in the

head, probably in the base of the cranium. A serous effusion would most likely, although it is not a necessary condition (for effusions of serum appear at times to be almost as sudden as effusions of blood), have been preceded by signs of cerebral disease. From the suddenness of the attack, and the absence of prior cerebral disturbance, the probability appeared to be that extravasation had taken place; the rapid amendment of those parts of the head, however, which were affected, seemed to militate against that probability, as absorption could scarcely be expected to proceed so rapidly; but the brain might in the interval accommodate itself to the pressure, and be less influenced by it, a change which might be equivalent to a diminution in the bulk of the deposit. The amendment which had already taken place in the condition of the patient, for whom aperient medicines had been prescribed, accompanied by a low plain diet, gave reason to hope for a favourable result, more especially with regard to the inferior extremity.

Dr. F. Bird said, that the case related by Dr. Chowne recalled to his mind a case which he considered to be one of serous apoplexy. He had seen a man attacked with hemiplegia who was cupped, and recovered completely in the course of the day, and was again attacked in the same manner a week afterwards, and on being cupped again recovered in three hours. He considered this must have been a case of effusion of serum, otherwise the recovery could not have been so rapid.

Mr. Snow thought that the hemiplegia in the case related by Dr. Bird had been caused by congestion of the brain, as blood-letting would, probably, not so soon have caused the removal of an absorption of serum. He related the case of a plethoric coachman, who, on two occasions, was immediately relieved from a state of complete coma by the abstraction of a basinful of blood.

Dr. Reid said, that congestion of the brain might cause all the other symptoms of apoplexy, but it did not cause hemiplegia.

LONDON MEDICAL SOCIETY.

Feb. 27th.—The discussion on puerperal fever was concluded this evening. On reviewing the observations forwarded to us by our reporter, the chief apparent circumstance is the diversity of opinion prevalent among the debaters not only as to the nature, but also with regard to the symptoms and the treatment of the affection. Is it an inflammatory disease, or is it totally distinct from peritonitis? Will it bear bleeding, or is the smallest loss of blood inadmissible? On these two points much difference of opinion was manifested. Indeed, in the present state of our knowledge regarding the subject the first question should be, What is puerperal fever? One fact only respecting the disease was generally admitted, namely, its unquestionable contagiousness. The obstetrician who meets with one case of puerperal fever may generally expect others, no care that he can take being sufficient to prevent the affection from spreading.

NEW MODE
OF REDUCING MERCURY
TO FORM A FERRUGINATED PILL OF MERCURY.

To the Editor of The Lancet.

Sir,—The varying and unsatisfactory quality of our "blue-pill" has for many years attracted attention. In my second edition of the "London Pharmacopœia" I gave the outline of a formula for preparing it with sesquioxide of iron; the further experience of years enables me to offer this preparation to the profession as a boon to them and to the public, for it may be proved that the iron enters along with the mercury into the blood, and saves the wear and tear of the human body under its use. I will not, at this time canvass, because I doubt, the probability of the sesquioxide (in double equivalents) yielding up one atom of its oxygen to the mercury, as is the opinion of several chemists to whom I have submitted this compound. I am aware that other peroxides, when moist, will similarly combine and divide mercurial globules; and I will not now enter upon the series of incomplete experiments instituted by myself to prove that mercury will amalgamate with other metallic oxides, and in this state form double or triple salts with acids. I shall now keep only to the practical utility of my compound, and to its importance as a great remedial improvement.

In the spirit of a pharmaceutical chemist I should either exclusively manufacture and sell it over my own counter, or I should take out a patent for it; in that of a manufacturing chemist I should do the like, or keep it a secret and save a fortune by it; were I of the new school of "summi auctores" I should throw the mere shadow of it, chemical, mesmerical, magnetical, and homœopathical, into a modest pamphlet, proving that the era of medicine truly commenced in 1843, and that up to that time all were shadows, clouds, and darkness. I shall content myself with publishing the recipe, and recording a brief statement of my experience.

℞ *Ferri sesquioxydi*, 3i;
Hydrargyri, 3ij;
Confect. rosæ Gallicæ, 3iij. Contere donec globuli non amplius conspiciantur.

It is made in five minutes; common blue-pill demands a week. The globules are not visible, even by the microscope. It is uniform in its appearance and effects. It makes a smoother pill, retaining its form more per-

manently. It salivates in a few days in the usual doses. The presence of the iron prevents the wear and tear of the human body under the effects of the mercury. It is particularly eligible for the strumous, the irritable, and for reduced anemial constitutions requiring mercury. The powers of life are not so much (scarcely at all) prostrated under its use. Its resolvent power is greater than that of mercury alone, especially with respect to buboes. Practitioners will at all events know what they are using; at present they have for blue-pill all manner of alloys and sulphurets,—mercurial-zinc pill, mercurial-sulphur pill, &c. &c. &c.

Five grains of sesquioxide will suffice to amalgamate and divide a large quantity of mercury, but I propose the larger proportion as a remedy. Your obedient servant,

G. F. COLLIER.

Spring-gardens, Feb. 27, 1843.

NOTE FROM DR. ROBERT LEE.

To the Editor of THE LANCET.

SIR,—In the last number of your Journal, at page 828, there is a note to Mr. Girdwood's paper on menstruation, which contains an insinuation against me, which I feel compelled to notice, as it is totally unfounded. The following is the note:—"Dr. Robert Lee refers to an 'Essay on the Periodical Discharge of the Human Female.'" London, 1832. I have not been able to meet with this essay by itself, but refer to it as published in 'Essays on the Female Economy' by Dr. Power, 1821. Indeed, I understand from the author himself that no other edition than the one of 1821, is he aware of ever having been published." I am very unwilling to believe that when Mr. Girdwood wrote this note he intended that the numerous readers of THE LANCET should suppose that I had been guilty of quoting from a work which I had never seen, and which in reality had no existence. The effect of this statement is, however, obviously calculated to excite this suspicion. In 1832, Burgess and Hill, Great Windmill-street, published an edition of the essay in question, with the title exactly as quoted by me in the "Cyclopædia of Practical Medicine," vol. iii., page 227, as follows:—"An Essay on the Periodical Discharge of the Human Female, by J. Power, M.D. 1832." I have put the work into the hands of Mr. Churchill, that Mr. Girdwood may have an opportunity of seeing it, and having his curiosity fully gratified. I remain, Sir, your obedient servant, ROBERT LEE.

March 6, 1843.

*** The above work, as described by Dr. Lee, is in the possession of Mr. Churchill, who will be happy to show it to any gentleman.

ROYAL COLLEGE OF SURGEONS IN LONDON.

LIST of gentlemen admitted members on Friday, March 3, 1843:—J. G. Devis, F. Bellamy, P. T. Kempson, F. W. Osborne, A. Lamb, C. N. Justan, T. Worth, W. Paterson, S. Payser, J. Shaw, G. Fisher.

LITERARY NOTICE.

The Life of a Travelling Physician, from his first introduction to practice; comprising twenty years' wanderings through the greater part of Europe, with notes of events, descriptions of scenery, and sketches of character. 3 vols, post 8vo. This work is the real life of an English physician. It consists of a journal commenced to relieve the *ennui* of a dull foreign court, and continued until the author's recent return to England.

BOOKS RECEIVED.

The Physiological Anatomy and Physiology of Man. By R. B. Todd, M.D., and Wm. Bowman, F.R.S., King's College, London. Part I. J. W. Parker, 1843. 8vo, pp. 200.

The People's Phrenological Journal. Part for March, 1843. London: Sherwood and Co.; and the Illustrated Polytechnic Review, March 4.

NEWS AND CORRESPONDENCE.

A Constant Reader would be liable to prosecution under the Act named for "prescribing and compounding his own medicines," although he made no charge for the articles supplied. The grievance is a large one, but it helps to make medical reformers. Would to God men fretted as much for the public good as on their own behalf.

Mr. H.—The "medico-philological curiosity" is precisely the contemptible sort of rubbish that such a silly fellow would be likely to produce, but his "So help me bread!" is not worth a place in these columns, even to expose his folly and ignorance.

The remarks made by *Mr. James* on the case of "fatal blow of the lung" were received, and long since acknowledged and declined, from reasons then given in this part of our Journal.

S. A. M.—The landlord could strip the house under the distraint, and refuse to satisfy the demand of the assistant, whose only remedy is against the master; but if the latter were a bankrupt the assistant's wages must be paid before other, dissimilar, claims were discharged.

N. N.—There is no "treatise" in the Journal under such a title. The weakness may be the consequence of some affection, which should be specifically named in seeking for information as to the proper treatment.